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Some New Possibilities in Solar Research.

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FORTY years ago I began a series of visual and photographic studies of the sun. Recently I have returned to this work, after an interval of enforced absence. My instrumental equipment is now augmented by the addition of a spectrohelioscope, a simple device for monochromatic vision which we owe in its essence to the pioneers of solar research. This renders visible a host of important phenomena hitherto concealed from view by the overpowering brilliancy of the sun's disc. Chief among these are the radiating and absorbing regions of the hydrogen atmosphere, visible with the spectroscope in profile at the sun's limb, and revealed in projection by spectroheliograms of the disc, but now accessible, wherever they may be situated, to visual observation with instruments of moderate size.

All readers of NATURE who are familiar with its early volumes will remember the discoveries of Janssen and Lockyer in 1868. At the total solar eclipse of that year, Janssen, who was observing in India, was so impressed by the brilliancy of the lines in the spectrum of the prominences that he determined to look for them in full sunlight. He did so, and was at once rewarded. The bright light of the sky around the sun, which ordinarily hides the prominences from us, gives the spectrum of scattered sunlight. Entering the spectroscope through a slit from one to three-thousandths of an inch wide, it is spread out into a band from a few inches to forty feet in length, depending upon the dispersive power of the spectroscope. Its intensity may thus be reduced indefinitely. The bright lines of the gaseous prominences, however, are essentially monochromatic. High dispersion separates them more and more widely, without greatly weakening them. They thus become readily visible on a fainter background of dispersed skylight. The principle involved was first stated by Lockyer in 1866 and successfully tested by him in 1868, before the news of Janssen's discovery had reached Europe. This application of the spectroscope opened a new and productive era in solar physics, enlisting the active efforts of astronomers in England, France, Italy, Germany, and the United States.

Naturally these observers wished to see not merely the spectra of the prominences, but also their forms. In his well-known book, "Contributions to Solar Physics" (p. 578), Lockyer describes how the first steps were taken:

"In my first paper I gave some drawings procured by causing the slit to pass slowly over the prominence. By this means a number of sections of varying length was obtained, which, placed side by side, gave an idea of its shape. The slit then used was extremely narrow, and was radial to the sun's limb. It was obvious that in this way the true shape of the prominence could not be seen unless the slit were moved with sufficient rapidity to allow of persistent images. At the very outset Janssen and myself attempted to accomplish this, Janssen by giving a rotatory motion to a direct vision spectroscope, I by giving an oscillating motion to the slit, in which I was followed by Young, who afterwards expanded it."

Prof. Young's account of his instrument is as follows:

"The eye-piece of the instrument has an apparatus attached, which, however, thanks to the high dispersive power, I find unnecessary.

"It was early proposed by Janssen to use a vibrating or rotating slit in order to make visible the form of a solar prominence, but as Zöllner has shown, the mere opening of the slit answers just as well, the light of the protuberance being diluted to precisely the same extent in either case.

"It occurred to me in connexion with a suggestion of Professor Morton, that by interposing at the focus of the eye-piece a diaphragm which should move with the vibrating slit, the light of the neighbouring portions of the spectrum might be cut off and this dilution avoided. Mr. Clark has devised and constructed a very beautiful mechanical arrangement by which this simultaneous and accordant motion of slit and diaphragm is effected by the rotation of the small fly-wheel shown in Fig. 1.

"But I find, that although seen in this way, the prominences appear very bright; yet the working of the apparatus always causes a slight oscillation of the equatorial, which interferes with the definition of details, and I prefer to work with the slit simply opened." (Young, NATURE, Dec. 8, 1870.)

The introduction by Zöllner and Huggins of the wide slit method and the superb views it afforded of the prominences at the limb led to its universal adoption, and the oscillating slit was abandoned. Although Lockyer and Young often examined the spectra of sunspots and other regions of the photosphere, and repeatedly described reversals and distortions of hydrogen

and other lines, I can find no indication that the oscillating slit device was ever tested for the observation of the forms of eruptions or other phenomena on the disc. Nevertheless, the credit for building the first spectro-

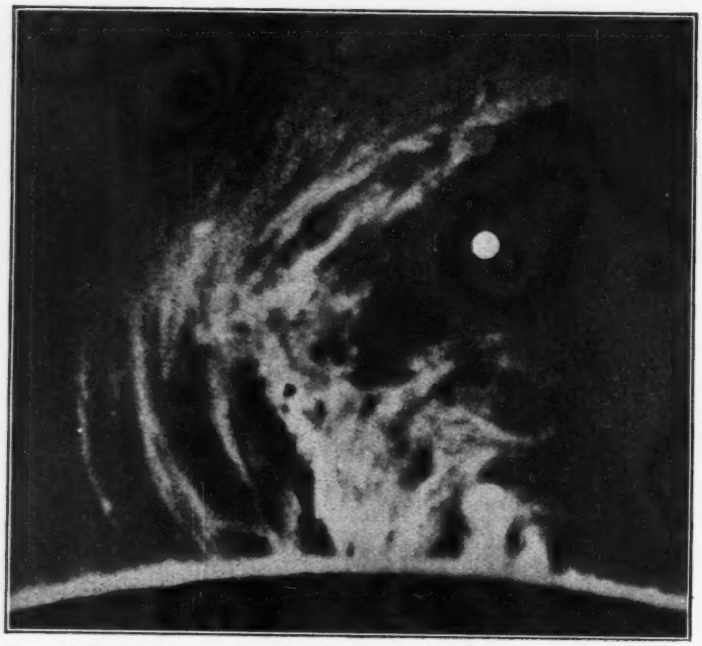


FIG. 1.—Active prominence, 140,000 miles high, photographed by Ellerman at Mount Wilson, July 9, 1917, with the *K* line of calcium. The small disc represents the comparative size of the earth.

helioscope (as I have named the instrument) and applying it to the observation of prominences at the limb belongs to my old friend Prof. Young, who was also the first to take photographs of the forms of the prominences through an open slit (*loc. cit.*).

THE SPECTROHELIOGRAPH.

The principle of the spectroheliograph, which does not differ greatly from that of Young's instrument with oscillating slit, occurred to me in 1889.[1] It involves the use of a spectroscope with a fixed second slit, through which a single line is admitted to a photographic plate. The whole spectroscope is mounted on steel balls, and moved slowly by a motor across the solar image, which, like the photographic plate, remains stationary. Or the spectroscope may be fixed in position, and the solar image and plate moved at the same speed across the first and second slits respectively. A monochromatic image of the sun is thus gradually built up on the plate from countless successive images of the narrow second slit.

After some preliminary experiments at the Harvard Observatory, I obtained photographs of prominences with a simple spectroheliograph at the Kenwood Observatory in the spring of 1891. In January 1892, with an improved spectroheliograph, I succeeded in photographing the chromosphere and prominences (Fig. 1) surrounding the sun and the bright flocculi[2] revealed on the disc by the use of the *H* or *K* line of calcium.

About this time Deslandres introduced the velocity spectrograph for photographing the *H* or *K* line in successive sections of the sun. Evershed soon constructed and systematically employed a spectroheliograph, and in 1893 Deslandres also began work with a spectroheliograph, which he employed for photography with the calcium lines and with some of the narrower dark lines. In 1903, with the Rumford spectroheliograph attached to the 40-inch refractor of the Yerkes Observatory, Ellerman and I first photographed the bright and dark hydrogen flocculi on the disc and noted especially the long dark flocculi (Fig. 2) identified as prominences projected against the sun (named 'filaments' by Deslandres, who has studied them extensively at Meudon with the spectroheliograph and velocity spectrograph). Five years later, at Mount Wilson, aided by plates sensitised for photography in the red, Ellerman and I discovered large vortices

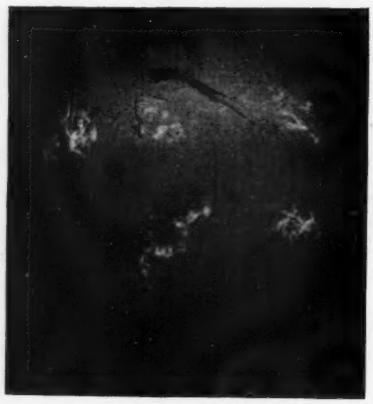


FIG. 2.—Hydrogen (*Hα*) flocculi, photographed with the 13-foot spectroheliograph at Mount Wilson, February 17, 1926, at $10^h\ 00^m$. All of these could be seen in Pasadena with the spectrohelioscope.

surrounding sunspots (Figs. 3 and 10). The H*α* line, with which the vortices were found, is much more

[1] I afterwards found that it had been first suggested by Janssen, embodied in a practicable design by Braun (not constructed), and unsuccessfully tested by Lohse.

[2] Flocculi is a general term applying to all markings, bright or dark (excepting sunspots), photographed by the spectroheliograph on the sun's disc.

effective than the more refrangible hydrogen lines for the study of the hydrogen atmosphere of the sun. Its situation in the red also adapts it for visual observations, particularly at low altitude stations where smoke and haze may seriously interfere with work at the violet end of the spectrum. With a spectroheliograph of high dipsersion, $H\alpha$ shows also the 'alignments' discovered by Deslandres, which constitute a reticular structure of wide mesh associated with the filaments.

The spectroheliograph, in addition to providing a method of photographing the chromosphere and prominences at the sun's limb, has thus made possible the daily study of the solar atmosphere in projection against the disc. But useful as it has proved, the need for a similar means of visual observation has persisted for many years.

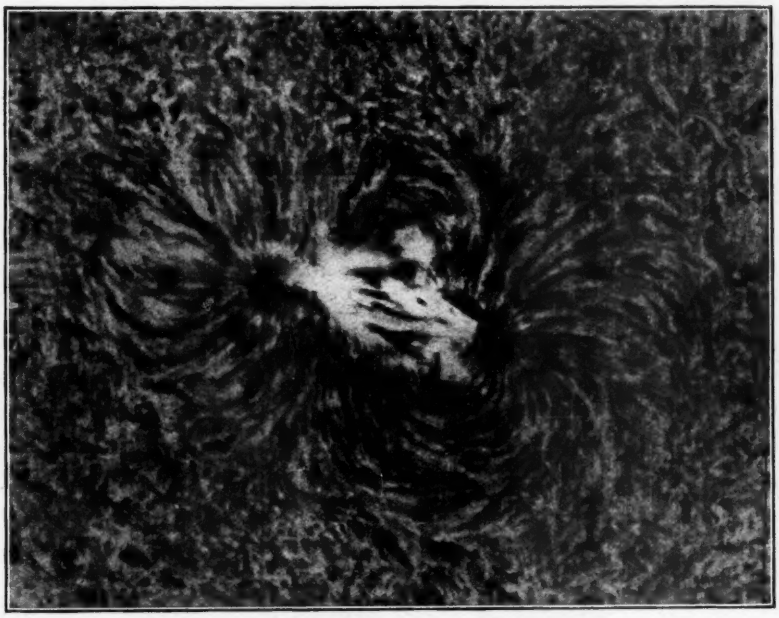


FIG. 3.—Hydrogen ($H\alpha$) vortices surrounding the bipolar sunspot of September 1924, photographed by Lewis Humason at Mount Wilson with the 13-foot spectroheliograph.

THE SPECTROHELIOSCOPE.

My first attempts to observe the hydrogen flocculi visually were made on Mount Wilson with the 60-foot tower telescope and 30-foot spectroheliograph soon after the discovery of the $H\alpha$ vortices in 1908. A disc carrying a large number of radial slits was mounted just above the wide first and second slits on a vertical bearing standing midway between them. Each of these radial slits, when near the axis of the collimator, thus produced a spectrum, and by adjusting the grating (or prism) the $H\alpha$ line could be brought into coincidence with the corresponding slit on the opposite end of the same diameter. A given displacement of the first slit produced an equal displacement of the $H\alpha$ line in the opposite direction. Thus, when the disc was rotating, the observer, looking through a positive eye-piece focussed on the slits coinciding with the $H\alpha$ line, saw an image of a portion of the sun in hydrogen light. Unfortunately, the slits were too wide and too few in number, and for many years I had no opportunity to bring the method into effective form.

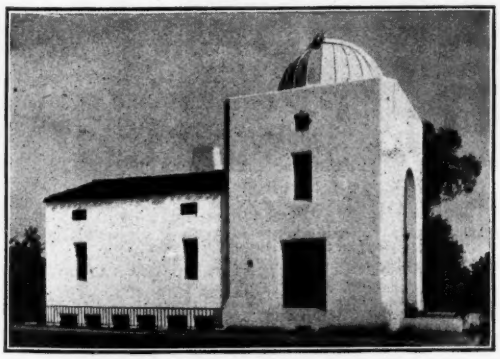


FIG. 4.—Solar Laboratory in Pasadena. The second mirror of a cœlostat beneath the dome sends parallel rays vertically downward to a 12-inch objective, giving a 2-inch solar image on the slit of the spectrohelioscope in the basement. By a simple change, a Cassegrain reflector of 18 inches aperture, giving a solar image either 6½ inches or 16½ inches in diameter, can be substituted for this objective.

In 1923 I renewed the effort, with results which have been described in these pages (NATURE, October 25, 1924, p. 628). At that time, however, the apparatus was crudely mounted, and its consequent inefficiency, at a period of low solar activity, gave me no measure of its actual powers. Recently the same optical parts, now adequately mounted in my new Solar Laboratory in Pasadena (a branch of the Mount Wilson Observatory), have yielded results of greater interest.

A very brief description of the instrument will suffice. Imagine a reflecting spectroscope of 6 inches aperture and 13 feet focal length, standing vertically in a well beneath the laboratory (Fig. 5). A solar image 2 inches in diameter is formed on the slit by a 12-inch objective.

A cœlostat, with second mirror, mounted beneath a dome at the summit of a low tower (Fig. 4) sends parallel rays of sunlight vertically downward to the 12-inch objective, which stands 18 feet (its focal length) above the slit. Electric slow motions, controlled by buttons within easy reach of the observer, permit the objective to be focussed and the solar image to be moved in any direction across the slit. Thus all parts of the limb or disc can be examined in quick succession.

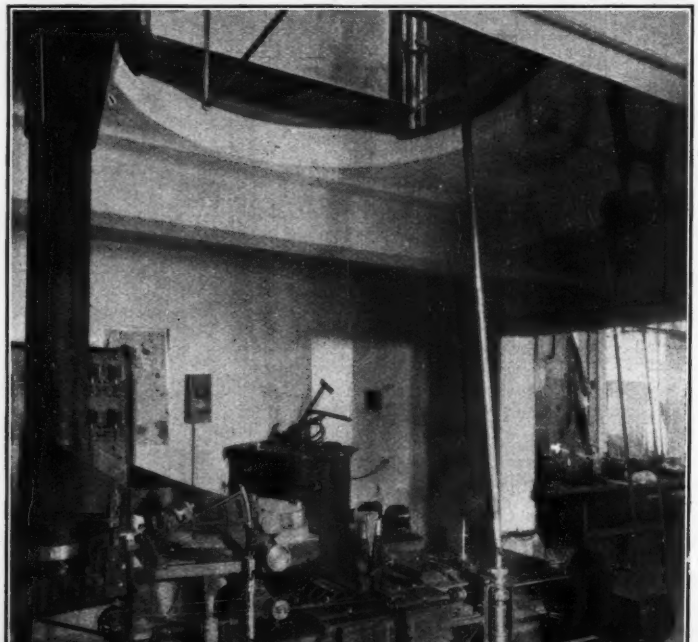


Fig. 5.—Upper end of combined spectrohelioscope, spectrograph, and spectroheliograph, mounted vertically in a well 10 feet in diameter and about 80 feet deep. The oscillating slit-bar and binocular eye-piece may be seen near the middle of the instrument. These are ordinarily employed with a reflecting spectroscope of 13 feet focal length, also designed for use as a spectroheliograph. By turning its optical parts out of the way, the instrument is transformed into a Littrow spectrograph of 75 feet focal length, suitable for researches on the magnetic fields in sunspots.

The second slit lies in the plane of the spectrum, formed by a large plane grating, ruled by Jacomini on the ruling machine of the Mount Wilson Observatory. The first order spectrum, which is very bright, is usually employed. The $H\alpha$ line is brought into coincidence with the second slit, which is narrower than the (dark) line. The slits are carried at opposite ends of a horizontal bar, which can be oscillated (usually through a range of a quarter of an inch) about a bearing half-way between them (Fig. 6). As the optical arrangements are such that the second slit constantly bisects the oscillating $H\alpha$ line, a monochromatic window, one-quarter of an inch wide, is thus provided through which to view the sun. A positive eye-piece magnifying about 2·5 diameters, mounted on the binocular body of my Spencer microscope, is used for observing the hydrogen atmosphere through this window. The speed of the driving motor is fast enough to give a persistent image. As the (visible) length of the window thus employed is about half an inch, a considerable portion of the 2-inch solar image can be seen at once.

I was agreeably surprised when I first tried this apparatus on January 16 of the present year. I knew from previous experience that the prominences at the limb and the more conspicuous dark and bright flocculi on the disc would be visible, but I was scarcely prepared for the delicate details of structure, both dark and bright, which appeared in good contrast on the disc. At that time (as in the earlier work) I was using five

slits at each end of the bar, instead of one. These were only one-twelfth of an inch apart, restricting the field of view to this width. The central image in the eye-piece, corresponding to the *Hα* line, was flanked on either side by brighter images of the same region,

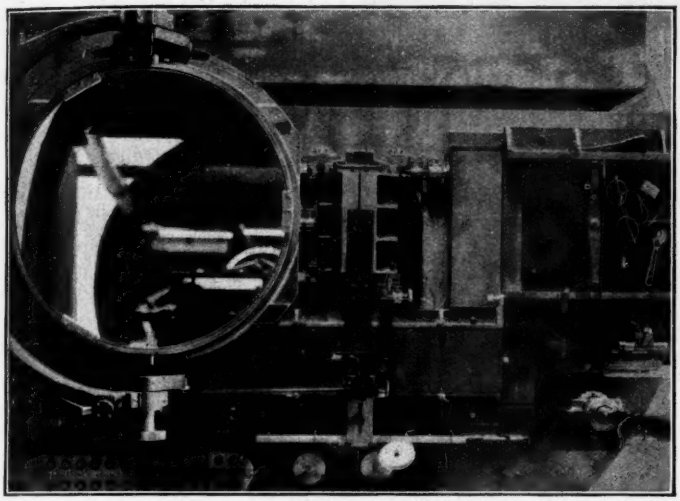


FIG. 6.—Plan of spectrohelioscope. The oscillating slit-bar, mounted on a bearing movable by the micrometer screw near the centre of the photograph, is driven by a motor at the left (shown in Fig. 5). The observer looks through the binocular eye-piece, which is focussed on the second slit. The 18-inch concave mirror of the Cassegrain reflector, the driving mechanism and sliding plate-holders of the spectroheliographs, and the buttons for electric control, may also be seen in the photographs.

formed by the light of the continuous spectrum. Thus, in examining a spot region the hydrogen flocculi above and about the spots could be seen in the central image while the underlying spots and photosphere appeared in the side images—a convenient means of comparison. Frequently in the hydrogen image the spots are entirely concealed by flocculi, and in tracing the spiral structure in a vortex to its objective, it is advantageous to be able to see the spots simultaneously. For this and other reasons two or more second slits may sometimes be preferred to one. By means of a special device, however, similar comparisons can be easily made with only one second slit.

Exploring the Solar Atmosphere.

As a quick and effective means of exploring the entire solar atmosphere the spectrohelioscope is unrivalled. In a few minutes one may run completely around the limb, not only observing the chromosphere and prominences projecting beyond it, but also following with equal ease those which extend on to the disc and appear there as dark absorbing masses. Such a prominence, from a spectroheliogram taken some years ago by Ellerman on Mount Wilson, is illustrated in Fig. 7. It will presently appear why objects of this nature can often be better seen with the spectrohelioscope than they can be photographed with the spectroheliograph.

The utility of the spectrohelioscope as a scouting auxiliary of the spectrograph or spectroheliograph is thus apparent. Hitherto the observer with these instruments has often worked blindly, not knowing in advance what phenomena of the solar atmosphere lie between him and the visible structure of the photosphere. A few minutes of visual observation may now enable him to select interesting regions for special study, or to avoid areas where eruptions, vortices, or other active phenomena may interfere with measurements requiring a more normal state of tranquillity. Such selections can also be made with the spectroheliograph, but far less quickly and effectively. The fact that very different phenomena are observed when the second slit is set on different parts of the *Hα* line is a vital factor here.

Effects of Level and of Radial Motion.

In our early work with the spectroheliograph, M. Deslandres and I independently utilised the principle of photographing cross-sections of the calcium flocculi at different levels by setting the second slit in successive exposures at various distances from the centres of the *H* and *K* lines. As these lines widen in descending through the calcium atmosphere toward the photosphere, it is obvious that if the second slit is set at a given distance from the centre, none of the higher

FIG. 7.—Solar prominence partly projected upon the disc, photographed by Ellerman at Mount Wilson with the 13-foot spectroheliograph (using *Hα*) May 22, 1916. At the left is a longer exposure of the same prominence, made with the disc covered.

calcium vapour, which gives a narrower line, can be included in the photograph. Similar work was afterwards undertaken with the hydrogen lines, where the second slit compatible with good definition and contrast.[3] But the discriminating power of high dispersion is very advantageous in many cases, and a means of utilising it without the limitations incident to the simultaneous use of only two second slits, or the delays involved in making a series of exposures like that in Fig. 8, is clearly desirable, especially in the case of rapidly changing eruptions. This is afforded by the spectrohelioscope, because of the ease of quickly shifting the position of the $H\alpha$ line on the second slit while observations are in progress. Of the various devices I have employed for this purpose, perhaps the simplest is a piece of plane parallel glass, mounted on trunnions just below the oscillating second slit. When this glass plate is tipped up or down, the line is displaced to red or violet. A scale in the eye-piece shows the exact position of the line on the slit. This is readily converted into a 'blink' apparatus for comparing the images of the flocculi corresponding to the red and violet edges of the line. An electromagnet for tipping the plate and two adjustable stops to define its limits serve for this purpose.

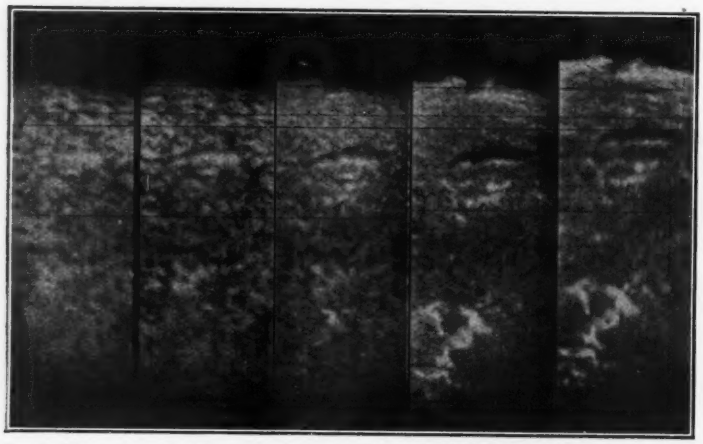


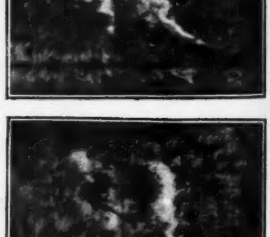
FIG. 8.—Five exposures by Ellerman with the 13-foot spectroheliograph, showing the changes in the flocculi corresponding to second-slit position, ranging from λ6564·36 (left) to the centre of $H\alpha$ (May 29, 1916).

velocity of the gas in the line of sight is often of greater significance than its level.

Fig. 8 illustrates the different appearances of a given region of the solar atmosphere when photographed with the second slit at various distances from the centre of $H\alpha$. Many studies of this kind have been made at Mount Wilson, and these have been supplemented by other investigations based upon photographs taken with a single first slit, an optical system giving two spectra of the same region of the sun, and two second slits, one set on the red side of $H\alpha$ in one spectrum, the other on its violet side in the second spectrum, equidistant from the centre of the line. Two photographs of the same region of the hydrogen atmosphere thus obtained simultaneously often show remarkable differences, as Fig. 9 illustrates. In this picture of an active spot region the bright eruptive flocculi appear in both photographs, because the corresponding bright $H\alpha$ line was wide enough to overlap both of the second slits. The dark flocculi, produced by the cooler gas at a much higher level, gave a narrower line, displaced to the red because of the rapid descent of this gas. The structure of this rapidly falling gas is consequently shown only in the lower photograph, made with the second slit set on the red side of the line.

The power of the spectroheliograph thus to single out gaseous masses moving rapidly in the line of sight is one of its most valuable properties. This effect can be minimised, if it is desired to record on a single plate all bright and dark flocculi moving at moderate radial velocities, by using the lowest dispersion and widest

With a device of this kind, or with a micrometer screw which permits the oscillating bar (and thus the second slit) to be displaced toward red or violet (shown near the centre of Fig. 6), it is fascinating to watch the changes in the appearance of the flocculi, and to distinguish between ascending and descending gases. In fact, we have here a special means of analysing the $H\alpha$ line and of separating the complex elements of which it is composed. Areas in the solar atmosphere which give rise, for example, to bright $H\alpha$ lines dis-



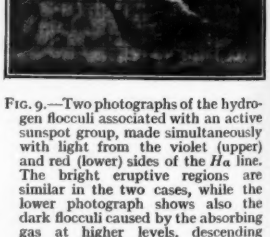


FIG. 9.—Two photographs of the hydrogen flocculi associated with an active sunspot group, made simultaneously with light from the violet (upper) and red (lower) sides of the $H\alpha$ line. The bright eruptive regions are similar in the two cases, while the lower photograph shows also the dark flocculi caused by the absorbing gas at higher levels, descending toward the photosphere.

[3] Or (if higher dispersion is employed) the $H\alpha$ line may be caused automatically to move back and forth across the narrow second slit during the exposure.

placed toward the red frequently lie in the line of sight or in such close proximity that they cannot be distinguished in photographs of the *Hα* line in the spectrum of the region. Thus in measuring the displacement of this line near spot groups or in other parts of the sun's disc we are dealing with an integrated effect, representing the superposition of the various *Hα* lines given by the superposed or closely contiguous areas in question. With the spectrohelioscope many of these areas can be separately distinguished and their radial velocities determined by moving the line back and forth over the second slit and noting the displacement from its centre, towards red or violet, when a given bright or dark structure reaches its maximum of intensity. Furthermore, the width of the bright or dark *Hα* line corresponding to any detail of structure can also be measured by taking the scale readings, to red and violet, at which this structure disappears. Indeed, it would not be difficult, by a simple photometric device, to determine the exact intensity curve of the line given by any of the small flocculi.

I am referring to observations within the boundaries of the dark *Hα* line and its two wings (each of which is about as wide as the dark line itself), and not to the large local displacements which are often seen in or near active spot groups. It should be added, however, that the spectrohelioscope is also well adapted for the observation of the rapidly moving gases which give rise to these large displacements. The form of the gas moving at any given velocity can be seen by setting the oscillating slit at the corresponding distance from the centre of the line.

FIG. 10.—Hydrogen (*Hα*) vortex photographed by Benioff at Mount Wilson with the 13-foot spectroheliograph. Simple vortices of this character are selected for determining the law of storms in the solar atmosphere.

PROMINENCES, FILAMENTS, VORTICES, AND ERUPTIONS.

Even for the observation of prominences projecting outside the limb, the oscillating slit has some advantages over the wide slit. This is because the former shows them in essentially monochromatic light, and thus often permits details to be seen which are confused with a wide slit because of the superposition of gaseous masses moving at different velocities in the line of sight. By setting the oscillating slit on different parts of the line, portions of the prominence moving at different velocities can be seen separately, as in the case of the flocculi just cited. Prominences in which there are marked internal differences of radial velocity sometimes do not appear at all on the disc at the slit position which shows them best outside the limb. A turn of the micrometer screw, however, suffices to reveal them in strong contrast, either as filaments or in other forms.

This suggests the use of a spectrohelioscope as a guiding device for a spectroheliograph, permitting the *Hα* line to be frequently readjusted on the second slit of the latter instrument during exposures, so as to record all phenomena at the limb and on the disc in strongest contrast. The spectrohelioscope itself may also be used for photography, either in the form described here, provided with an auxiliary camera and right-angle prism for visual observations between exposures on small areas; or as a spectroheliograph with oscillating slits, capable of photographing the entire solar disc. With the former arrangement, the changes in the appearance of given areas when *Hα* is moved across the second slit may be recorded, perhaps even in the form of moving pictures.

I cannot dwell, however, on the remarkable phenomena thus brought to view. Some of them are illustrated in Figs. 2 and 11, but these spectroheliograms are quite inadequate to show the delicate details,[4] and they of course fail to indicate the effect of shifting the line on the slit. Brief reference should be made, however, to observations of vortices and eruptions.

I have previously described in NATURE our researches

[4] This remark refers to our spectroheliograms with 2-inch solar image. The details of the vortex structure are best shown by our spectroheliograms of the 6½-inch solar image given by the 60-foot tower telescope on Mount Wilson (see Figs. 3 and 10). I am not yet certain whether the most delicate details on such photographs can be so well seen with the spectrohelioscope.

at Mount Wilson on the law of sunspot polarity.[5] This is part of a more general investigation on the law of whirling storms in the sun, which we find to be much more complicated than the terrestrial law. The bipolar vortices which constitute typical sunspots are of opposite polarity in the northern and southern hemispheres of the sun, and these polarities are reversed at each sunspot minimum. The available evidence favours the view that this magnetic change is due to a reversal in the direction of whirl in the vortices that produce the spots and their magnetic fields. These vortices in the photosphere are accompanied by secondary vortices in the overlying atmosphere, recorded (as in Fig. 10) by the spectroheliograph. From a preliminary study[6] it appears that the direction of whirl in about 80 per cent. of the high-level vortices accompanying single spots (or the preceding spots of bipolar groups) corresponds with that of terrestrial storms, and does not reverse at sunspot minima.

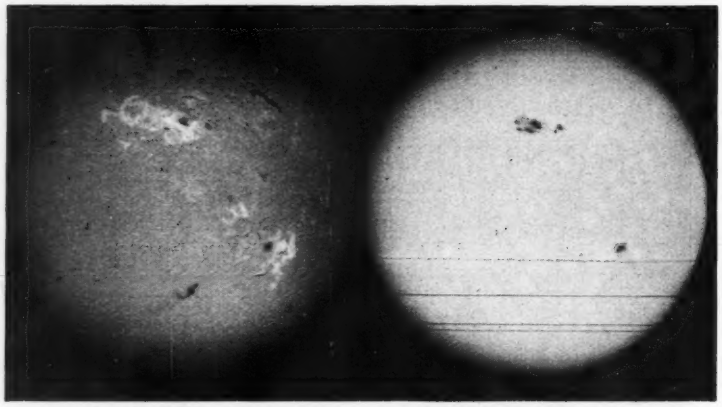


FIG. 11.—Bright eruption in great sunspot group at 22° N. latitude, photographed at Mount Wilson with the 13-foot spectroheliograph, January 24, 1926, at 9h 34m Pacific standard time. The image on the right is a direct photograph of the sun, obtained simultaneously with hydrogen ($H\alpha$) image on the left. This eruption was detected independently in Pasadena with the spectrohelioscope.

The above results were derived from an examination of spectroheliograms, but I have found the spectrohelioscope a very useful means of continuing the investigation. If, as often happens, a prominence (or filament) occurs as a dark flocculus near a spot, its spiral form may at once indicate the direction of whirl in the vortex.[7] Owing to differences in radial velocity, a slight change in the position of the $H\alpha$ line on the oscillating slit is frequently necessary to show such a vortex clearly. For detailed studies of vortex structure and changes of form, both in plan on the disc and in cross-section at or near the limb, the spectrohelioscope has also shown its efficiency.

This instrument is also of great service for the detection of such violent outbursts on the sun as I observed on January 24 (Fig. 11) and 25, 1926, preceding the great aurora and magnetic storm of January 26 and 27 already described in NATURE (February 6, p. 208). The eruption, which continued for several hours on January 25, surpassed in brilliancy all solar phenomena I have ever before seen, and exhibited rapid changes of form.[7] Because of the ease of glancing occasionally during the day at probable regions of activity, and of instantly finding the best possible position of the second slit on $H\alpha$, the spectrohelioscope should greatly assist in determining the true relationship between solar eruptions and terrestrial auroras and magnetic storms. If, as I hope, an inexpensive instrument can be built for the use of amateurs, the chances of detecting the exact moments of critical outbursts will be greatly multiplied. A small spectrohelioscope for this purpose, and improved oscillating and rotating slits to replace the oscillating bar hitherto employed with the 13-foot spectrohelioscope, are now under construction and will soon be ready for trial.

[5] "Sunspots as Magnets, and the Periodic Reversal of their Polarity," NATURE, January 19, 1924, p. 105.
[6] Hale, "A Test of the Electromagnetic Theory of the Hydrogen Vortices surrounding Sunspots," *Proc. Nat. Acad. Sciences*, **11**, 691-696, 1925.
[7] For a brief description of these observations, and some suggestions regarding other applications of the spectrohelioscope, see "Visual Observations of the Solar Atmosphere," *Proc. Nat. Acad. Sciences*, **12**, 286-295, 1926.

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Scientific Research and Service.[1]

By H.R.H. The Prince of Wales, K.G., D.C.L., F.R.S.

MY first duty, as president of our great Association, must be to read to you the following message from His Majesty The King:

I am sensible of the distinction conferred upon my dear son, The Prince of Wales, in presiding at this year's meeting of the British Association for the Advancement of Science; for I realise that no member of my family has occupied this position since my grandfather was president in 1859. I cannot do better than repeat the assurances then made by the Prince Consort on behalf of Queen Victoria, and express my deep appreciation of the all-important and ceaseless labours in the cause of science of those eminent men who enjoy the membership of your world-renowned society.

I propose on behalf of the Association to forward the following reply to this message:

The members of the British Association for the Advancement of Science assembled at Oxford humbly beg to express to Your Majesty their loyal appreciation of the patronage extended to the Association by your Father and Yourself, and of Your Majesties' repeated expressions of personal interest in its work.

The advancement of science is the constant object of the British Association; to give a stronger impulse and more systematic direction to scientific inquiry, to promote the intercourse of those who cultivate science in different parts of the British Empire with one another and with foreign philosophers, to obtain a greater degree of national attention to the objects of science, by removing those disadvantages which impede its progress, for the well-being of Your Majesty's realm and the general good of mankind.

My second duty is to try to tell you—if this be possible—something which you do not know already. I must admit frankly that, for a long time, the prospect of attempting this has weighed on me heavily. For a man who, along with the great majority of his fellow-creatures, can lay claim to no intensive scientific training, it is no light responsibility to be called on to address the annual gathering of the British Association. But, believe me, I do not intend to shirk that responsibility; for it seems to me that only by discharging it as well as I possibly can, shall I be able to show you how highly I value the great honour you paid me when you added my name to those of the distinguished men who have been your presidents in past years.

At first sight it might appear a hopeless task for any one who knows nothing of science to talk to you, who know everything about science. But those who work in the scientific field will be the first to admit that no task is really hopeless, and, when I approached this one, I began to think I might perhaps find a few topics in which I could interest you. For, after all, science is only another name for knowledge, and any man who goes about the world with his eyes open cannot fail to acquire knowledge of some sort, which, if he can express it, must appeal to any audience.

To adapt one of our most familiar sayings, the onlooker can see a great deal of the game. I, for example, though I claim no insight into pure science, can fairly claim an onlooker's experience of very many practical examples of science as applied to the needs of our civilisation to-day. For some years past, in war and in peace, I have been privileged to have countless opportunities of examining, at close quarters, the concrete results of such applied science. In things military and naval, in factories, workshops, mines, railroads, in contact with the everyday problems of education, health, land-settlement, agriculture, transport or housing—in all such varied departments of human life—it has been borne in on me more and more that if civilisation is to go on, it can only progress along a road the foundations of which have been laid by scientific thought and research. More than that, I have come to realise that the future solution of practically all of the domestic and social difficulties with which we have to grapple nowadays will only be found by scientific methods.

It is from this experience, and with the convictions it has brought, that I should like to tell you something of my general impressions of the bearing of scientific research on the daily life of the community; and to show how that relationship can be developed by the mutual co-operation of scientific workers and the State. I cannot better embark on this attempt than by quoting to you the words of my distinguished

[1] Presidential address delivered to the British Association at Oxford on August 4.

predecessor, though without the hope that what follows will maintain the high standard which he set in his presidential address at the last meeting.

Prof. Lamb, on that occasion, expressed confidence that the efforts of scientific workers "have their place, not a mean one, in human activities, and that they tend, if often in unimagined ways, to increase the intellectual and the material and even the æsthetic possessions of the world. And in that assurance," he continued, "we may rejoice that science has never been so widely and so enthusiastically cultivated as at the present time, with so complete sincerity, or (we may claim) with more brilliant success."

This claim, by no means exaggerated, invites reflection upon the intimate association of the results of scientific research with the daily lives and affairs of every one of us. It is a good thing to reflect upon this, even for those who have no sort of direct contact with scientific research, if only because the doing so may dispel an attitude towards science, which personifies it somewhat as the ancients personified the powers of darkness, and invests it with some of their sinister attributes. Such an attitude of mind is fortunately less common than it used to be. Prof. Lamb, in the address already quoted, referred to a certain feeling of dumb hostility toward science and its works, which still survives. No doubt it does; but at least it has ceased to be vocal, as it was in the earlier days of the Association. It became loud, for example, at two of the meetings in this very place. The later of these two occasions was the Oxford meeting in 1860, and the field of battle was the Section of Botany and Zoology, in which the theories put forward in Darwin's "Origin of Species" were debated, in a manner which has passed into history, between Wilberforce, Bishop of Oxford, on one hand, and Huxley and Hooker on the other.

The earlier occasion, however, more appropriately illustrates, by contrast, the modern realisation of our debt to science. The second meeting of the Association, in 1832, took place in Oxford. The University was not, at that time, without distinguished cultivators of science. The invitation to Oxford came from Charles Daubeny, who combined the professorships of chemistry, botany, and rural economy, and the president was William Buckland, then Canon of Christ Church and professor of mineralogy and geology. A strong body of opinion resented the recognition of science by the University when carried to the extent of conferring honorary degrees upon four of the distinguished visitors. The famous Keble, moved for once to anger, referred to those who were thus honoured as a "hodge-podge of philosophers." Their names were David Brewster, Robert Brown, John Dalton, and Michael Faraday. Each of these men has left in the history of his own special branches of science an outstanding memorial. Brewster's researches into optics were his greatest scientific achievement; to our own gratitude he has an especial claim as the leader among the founders of our Association. Brown's services to botany were unsurpassed; perhaps that of widest appeal is his very thorough investigation of the flora of the coastlands of Australia, made during the voyage on which he accompanied Flinders in 1810–14—an early example of what may be termed imperial research. Dalton's name is identified for ever with the atomic theory, and he placed meteorology on a scientific footing. Faraday's labours provide one of the most wonderful examples of scientific research leading to enormous industrial development. Upon his discovery of benzene and its structure the great chemical industries of to-day are largely based, including, in particular, the dyeing industries. Still wider applications have followed upon his discovery of the laws of electrolysis and of the mechanical generation of electricity. It has been said, and with reason, that the two million workers in Great Britain alone who are dependent upon electrical industries are living on the brain of Faraday; but to his discoveries in the first instance many millions more owe the uses of electricity in lighting, traction, communication, and industrial power.

Oxford, then, was not dishonoured in the hodge-podge of philosophers whom she recognised in 1832. Nor will she recall with any disfavour the singularly doubtful compliment paid her on that occasion by another distinguished visitor, in whose mind the opposition must have rankled; the University, he said, had prolonged her existence for a hundred years by the kind reception he and his fellows had received. The Association will scarcely make that claim to-day; but its visiting members will have ample opportunity to learn how, through her museums and laboratories, Oxford, within the hundred years thus tolerantly allotted to her, has kept pace with the scientific development of the period. It need surely be no matter for regret if science has worked for and is taking a place, not only in the university but also in the schools, complementary with that occupied by the humanities. For complementary these two branches of learning must ultimately be. All the greatest exponents of scientific learning have been men of attainment also in letters.

The services rendered to mankind by the labours of outstanding figures in science, such as Faraday, or Kelvin, or Pasteur, or Lister, are matters of too common knowledge to need insisting upon in this place. What is perhaps less generally appreciated is the extent to which, through the efforts of very numerous workers, the results of scientific research have been brought to bear upon many of the most pressing domestic and industrial problems of the day, and that the co-operation between the laboratory and the State (which means the community) has been greatly strengthened of recent years. The British Association has always supported such co-operation. One of its principal aims, as stated by its founders and maintained ever since, is "to obtain more general attention for the objects of Science and the removal of any disadvantages of a public kind which impede its progress." In an article contributed by Brewster to the *Quarterly Review* in 1830, he asserted frankly that "the sciences of England" were "in a wretched state of depression, and their decline is mainly owing to the ignorance and supineness of the Government" as well as to various other causes which he detailed. The same theme (if less forcibly stated) recurs in some of the earlier addresses from the chair of the Association: the Prince Consort, for example, as president in 1859, thus indicates his view of the situation at that time: "We may be justified in hoping," he said, "that by the gradual diffusion of science, and its increasing recognition as a principal

part of our national education, the public in general, no less than the legislature and the State, will more and more recognise the claims of science to their attention; so that it may no longer require the begging-box, but speak to the State, like a favoured child to its parent, sure of his parental solicitude for its welfare; that the State will recognise in science one of its elements of strength and prosperity, to foster which the clearest dictates of self-interest demand."

It may be fairly said that the position foreshadowed in those words is now, in a large measure, attained. The progress towards it was visible, if slow, down to the end of the last century; but the beginning of a new era was then marked by the establishment of the National Physical Laboratory. This was at first set up in Kew Observatory, a building which, as a laboratory for magnetic and meteorological observations, and for the standardising of instruments, owed its maintenance to the British Association for thirty years from 1841, when, as a Royal Observatory, the Government decided to dismantle it. The building proved incapable of extension to accommodate the whole of the work, and in 1900 Bushy House, Teddington, was placed at the disposal of the laboratory by the Crown. The Laboratory, at its inception, was divided into departments dealing with physics, engineering and chemistry, and it possesses also the famous William Froude experimental ship tank. The investigations with which it has been so largely concerned—the testing and standardisation of machines, materials, and scientific instruments, researches into methods of measurement with the utmost accuracy, work on scale-models of ships, and the like—while of the first importance to Government Departments concerned with such applications of science, have also achieved many valuable results for industry in improving standard qualities, in indicating scientific methods applicable throughout a variety of manufactures, and thus in bringing about an improvement in the quality of their output for the benefit of consumers—which is to say, ourselves.

In historical sequence among the events which have strengthened interaction between science and the State, there follows the establishment of the Development Commission in 1908. Until that date the only agency for agricultural research in Great Britain was the classical experimental station at Rothamsted, a private benefaction; and the expenditure of the State on this prime factor in national economy was trifling. Since 1908 the Rothamsted station has been expanded to cover the whole field of nutrition and disease in the plant, while other institutes have been founded to deal with other aspects of agriculture such as plant breeding, the nutrition and diseases of animals, agricultural machinery and the economics of the industry. Not only are these institutes providing knowledge for our own farmers, but they also form the training-ground for agricultural experts required by the Dominions, India, and the Crown Colonies, which need no longer look abroad for their advisers. At the plant-breeding institute at Cambridge, Sir Rowland Biffen has provided several new wheats, of which two are generally grown throughout England; the extra yield and value of these wheats must already have more than repaid the whole expenditure on agricultural research since the institute was founded.

Among other examples of the value of research there may be mentioned the discovery of a variety of potato immune from the ineradicable wart disease, which a few years ago threatened the principal growing districts. The clearing up of the confusion into which commercial stocks of fruit trees had fallen has ensured that growers may plant orchards upon uniform stocks suitable to the soil and climate. Among the most important inquiries are those into the production and cleansing of milk, which have resulted in an entire reform of rationing, increasing the yield of each cow by one to two hundred gallons a year, and in freeing milk from the risk of contamination with disease.

Research into fisheries, which are administratively associated with agriculture, has become a matter of necessity in the light of evidence that even the vast resources of the sea have their limit, and can be injured if they are not exploited with due care and knowledge. Great Britain, acting in co-operation with the other nations who share with us the northern seas, has accomplished much in ascertaining the causes of the fluctuating herring supply, and has contributed notably to the study of the methods by which the stocks of plaice can be maintained. Research again is active in finding methods by which we can mitigate one of the consequences of our dense population—the pollution of our rivers and estuaries—and a method has been found whereby great supplies of shell-fish that had been condemned are once more available as food. Many know, too, of the remarkable results obtained from the scientific study of the habits of the salmon. Though fishing has been described as "a fool at one end of a string and a worm at the other," the subject is not without its personal interest, I believe, to many learned men.

Reverting to the historical sequence, it is appropriate to recall, with gratitude for its labours, the constitution of the Medical Research Committee in 1913, under the Insurance Act of 1911: this has since (in 1919) been transferred to a committee of the Privy Council under the name of the Medical Research Council, and its funds are directly voted by Parliament instead of being drawn from the contributions made by or on behalf of insured persons.

Research alone could provide the knowledge on which must be based all wise and effective legislation or administrative action in the interests of the nation's health. Yet until 1913 the State had played at best a subsidiary part in the organisation of such research and the provision of its material support. Under the new conditions, the State is actively concerned with the promotion and co-ordination of medical research towards conquest of those infirmities with which ignorance has afflicted humanity. A few only may be mentioned, which have rightly appealed to wide public interest. Insulin, a gift to science and to humanity from young enterprise and enthusiasm in the Dominion of Canada, is not only saving lives that were threatened, and restoring almost to normal health and enjoyment many that were crippled by weakness and restriction, but also, as a tool of investigation, is shaping new knowledge that will influence all our ideas of the functions of the body, in health or disease. The discovery of the vitamins, those still mysterious and minute constituents of a natural diet, has brought

understanding of various defects of health and of development, created for us largely by the blindness of civilisation to dangers accompanying its progress, dangers which science can avert.

Closely linked with the discovery has been the more recent development of knowledge concerning the need of sunlight for health, in man and his fellow animals as in plants. We know now that crippling deformity appears in the growing child unless he receives his proper share of the vitalising rays of the sun, either directly or through the presence in natural foods of vitamins which these rays have produced. Sunlight, or its artificial equivalents, have some importance already in the treatment of disease; but a realisation of its significance for health has a much greater importance in preventive hygiene. There can surely be no plainer duty, for a State charged with the health of an industrial civilisation, than to promote with all its resources the search for such knowledge as this, as well as to provide for its application when obtained.

Among diseases which painfully affect the popular imagination, cancer has an evil pre-eminence, largely on account of its mysterious, and therefore seemingly inevitable nature. For many years past a volume of investigation, supported by private benefactions and organised charity, has patiently accumulated knowledge of the beginnings of cancer and the conditions of its growth. Now, at length, there are signs of more rapid progress towards a penetration of its secret. Patience and caution are as necessary as ever; a new and exacting technique is still in development; but there is a new spirit of hope and enthusiasm. It is reassuring to know that in this, as in other directions, the State is giving its direct support to investigation, and co-operating with the foundations due to private generosity.

Looking backward a dozen years or so, one may say that science was definitely, by that time, a working part of the machinery of the State, though, as we see now, not a part working at full power. The War caused a broadening, so to speak, of the scientific horizon, for men of science themselves in some measure, but for the layman in a measure far greater. We all were brought to recognise the applications of science as adding, it may be, in certain respects to the distresses of warfare; but also as immensely alleviating the sufferings caused by it, and as indicating many methods of strengthening the arts of defence—some of which methods are no less valuable in strengthening the arts of peace. The creation of the Government Department of Scientific and Industrial Research was an act which falls, historically, within the period of the War; but as an outstanding incident in the scientific advancement of national affairs, it certainly is not to be regarded as merely a war measure; it was once described as a near relative of "Dora," but that was a mistake. Nevertheless, by an odd freak of history, it needed the whole period of a century between one great war-time and the next—between the Napoleonic and the World Wars—to mature the conception of a State department of scientific research. Some idea of this kind was clearly present in the mind of Brewster, and certain of his contemporaries, concurrently with his idea of the foundation of our own Association in 1831; and later, in 1850, when he addressed the Association from the chair, he claimed a strong advance in scientific and public opinion toward his views. Five years later a concrete proposal for the creation of a Board of Science, possessing "at once authority and knowledge," was put forward by the Parliamentary Committee of this Association (a committee no longer existing); but our Council at the time considered that the proposal had "yet to receive sanction from public opinion, and more especially from the opinion of men of science themselves."

It was not, in fact, entirely owing to lack of prevision on the side of successive Governments that the developments which have been outlined were so long delayed. There was an element of mutual distrust between science and the State—now, it may happily be believed, almost if not quite wholly removed. A strong body of scientific opinion was avowedly afraid, as Sir George Airy phrased it, of "organisations of any kind dependent on the State." It is to be hoped that modern developments have removed that fear. The progress of science cannot be kept wholly within training-walls, and no one wants to try to keep it so. The waters of a river may be guided artificially to do the work of irrigation; but not at their sources, nor yet where, at the last, they percolate the soil. The guidance of scientific research, in its inception, lies with the genius of the individual; its results for the future may lie far beyond the realisation even of the scientific workers themselves. The Oxford meeting of the Association in 1894 supplies a simple example of this. There was a discussion on flight, in the Section of Mathematics and Physics, opened by Hiram Maxim; and no less a leader in science than Kelvin afterwards described Maxim's own flying machine as a child's perambulator with a sunshade magnified eight times. Yet it was not many years before research in aeronautics had become the care of the State as well as of the individual; and the work carried out before 1914 under what is now the Aeronautical Research Committee led on to our wonderful development of aircraft during the War.

A recent report of the Committee of the Privy Council for Scientific and Industrial Research shows that under the Department there are eleven research boards, some of which direct the work of committees to the number of three dozen in all. These boards co-ordinate and govern researches in chemistry, fabrics, engineering, and physics, radio, building, food-investigation, forest-products, and fuel; and to these are to be added the board of the Geological Survey and the executive committee of the National Physical Laboratory. Under the general supervision of the advisory council there are upwards of twenty industrial research associations, formed in alliance with the same number of the principal industries of Great Britain, for the purposes of scientific investigations connected with those industries.

No attempt can be made here to review the whole field of work of these various bodies; but a few examples may be chosen for the purpose of pointing out what may be called their homely application. First, then, as to the building of the home. The Building Research Board was created in 1920, and in 1925, at the request of the Ministry of Health, considerably extended its activities. Researches are concerned with the study of materials from the chemical and geological aspects,

their strength, weathering, moisture condensation on wall coverings, acoustics, and various other problems; these inquiries, together with the collection and supply of information both by publication and through an intelligence bureau, represent, as the report states, "an attempt to create a real science of building, to explain and supplement the traditional knowledge possessed to-day in the industry." It can scarcely be questioned that industrial Britain inherits a legacy of discomfort in the housing of its workers, with all which that implies, dating from a period when the building of the home lacked scientific as well as æsthetic guidance. We need that guidance no less to-day, when the saving of labour is one of the main objectives of the 'ideal home' and its fitments.

Next, a further word as to our food supplies. The Food Investigation Board directs committees concerned with meat and fish preservation, fruit and vegetables, oils and fats, and canned foods. There is also a committee for engineering problems associated with the investigations; conditions of storage have been investigated on ships between Great Britain and Australia, and problems of heat conductivity at the National Physical Laboratory, while chemical substances suitable for refrigerants have been studied at the Engineering School in Oxford. At Cambridge a low-temperature research station has been established on ground given by the University, and is working in co-operation with the University biochemical, botanical, agricultural, and other laboratories. As for the investigations upon fruit and vegetables, the report may again be quoted, for it illustrates in a sentence something approaching the ideal of scientific co-operation brought to bear upon one particular home necessity, and, what is more, upon a particular and important branch of Imperial commerce. "There is," it says, "a closely knit scheme of work, which rests, on the one hand, in university schools of botany, and, on the other, in commercial stores scattered all over the country, where accurate records of results and conditions have been kept, and extends to the conditions of transport by ship, and overseas so far even as the Australasian orchards."

Other directions of research which touch upon commonplaces of our daily life are those concerned with fuel, with illumination, with the deterioration of fabrics and the fading of coloured stuffs, and—perhaps most homely example of all—with the application of scientific methods in the laundry industry. This will be good news to those of us who may have suffered, or may even be suffering now, from the torture of a collar which comes back from the wash with an edge like a surgical saw.

It must be clearly understood that the few examples mentioned represent only a small fraction of the present activities of science in co-operation with the State, and, expressed as they are here expressed, they may appear to wear an aspect even of triviality, because they deal with common things. But it is precisely because they do deal with common things that they are not trivial. There may be matter for amusement in the fact that science is concerning itself with the contents of the clothes-basket; but there is also matter for congratulation, and there may, in the future, be matter for sincere gratitude. Scientific research, properly applied and carried out, is never wasted. It may prove that a thing can be done, or that it can not be done; but even the proof of a negative may save the waste of further effort.

This attitude of the State toward science makes for an easing of the paths for the advancement of science in many directions; it marks a definite step in human progress, taken after long hesitation, but in itself new; and because it is new, we may believe with some reason that we live, not merely in an age of science, but at the beginning of it. The movement for co-operation which we have been discussing is not confined to Great Britain. It has borne fine fruit already in other lands; and in particular it is active in our own Dominions. The Indian Empire stands in a somewhat different category from these: there is here a tradition, so to say, for the application of science in its government, and the scientific results of its census investigations, its surveys, its agricultural, forestry, and other administrative departments have long been famous. This is not to imply that brilliant scientific work has been wanting in the Dominions—far from it—but the co-operative movements with their governments have followed that in Britain and with a laudable promptitude.

The trend of developments following upon all these movements has been similar, broadly speaking; it is sought to take a comprehensive survey of the natural resources and industrial opportunities of each Dominion, to explore the means by which science may be best applied to their exploitation, to provide, whether in State institutions or in university and other laboratories, for the pursuit of the necessary researches, to co-ordinate the work, and to ensure the dissemination of knowledge acquired. The nature of the researches themselves is conditioned to a large extent (though by no means wholly) by geographical circumstances in the respective territories: agricultural, pastoral, and forestry problems, for example, are not identical in all of them, and that very fact adds to the interest and value of co-ordinating the results of research work throughout the Empire. While problems may differ, solutions may point to a common end. Nothing but good can follow from personal contact between scientific workers in different parts of the Empire. Nothing but good can follow from their researches if they add, as gradually they must add, to the wider knowledge of the Empire not only among the workers themselves, but ultimately among the whole body of informed Imperial citizenship; not only in the overseas territories, but also at home. For us at home the Empire is worth knowing. Our knowledge of it begins with the school lessons in geography and history—or should do so; no doubt the ideal here is yet to be attained. Such knowledge may become later of vital importance to those who wish to join the stream of overseas migration.

The British Association, in pursuit of its policy of obtaining from time to time "reports on the state of science" in one department or another, has recently, through a committee of the Section of Educational Science, been collecting evidence as to the facilities existing in our schools for training boys and girls for life overseas. In the crowded curriculum of most schools these facilities, at any rate in their particular Imperial application, are not conspicuous. Yet any labour which time allows us to spend, whether in

school days or after them, upon the advancement of scientific knowledge of the Empire, of the means and manner and environment of life in its component territories, must be well spent. The British Association has played its part in this advancement since, in 1884, it admitted the principle and established the practice of holding occasional meetings overseas. Those of our members who travelled from Great Britain to take part in these meetings have had peculiar opportunities to meet and discuss each his own scientific problems with fellow-workers in the Dominions—and it should be added with particular reference to those meetings which have been held in Canada that they have provided almost unique opportunities for personal contact between British workers in science and their American colleagues.

Our travelling members have been able to see how science is cultivated in the universities of the Dominions and in many other institutions; they have gained first-hand acquaintance with the special problems of one territory and another; and when they have returned home they have talked—as any one who travels the Empire is impelled to talk. I have myself been guilty of giving way to this impulse on more than one occasion. Opportunities for travel are none too common for most of us, but most of us can at least cast our minds back to the exhibition at Wembley. Science herself, as an exhibitor, took a place there befitting her natural modesty. The scientific exhibit arranged by the Royal Society, admirable as it was, was confined to two rooms of the Government Pavilion. But was not a very large proportion of the entire exhibition, in point of fact, an exhibition of applied science?

It is impossible in the Imperial connexion to overstate the case for science. Sir William Huggins, in his presidential address to the Royal Society in 1901, said that "assuredly not only the prosperity, but even the existence of this Empire will be found to depend upon the more complete application of scientific knowledge and methods to every department of industrial and national activity." To-day we see that application in much fuller progress than when Huggins spoke only a quarter of a century ago, and already we know how truly he prophesied.

It is not for a moment to be supposed, because the State has come to take a more active and practical interest in scientific research, that there is therefore any occasion for the lessening of interest on the part of societies and individuals. The State interest involves that other interest, and invites it. It can never become the exclusive function of the State to aid the individual research worker. The State may, and does, co-operate in aiding him, as, for example, through the universities and the Royal Society. Nevertheless, there are whole departments of research which do not come within the range of public assistance, but are no less valuable because they do not. Therefore the support of science remains the concern of our scientific societies, educational institutions, industrial organisations, and private benefactors, no less than it ever did; nay, the very fact that the State has lent its aid should encourage them to continue their aid and to reinforce it—indeed, there is satisfactory evidence that this actually happens. One example will suffice which indicates, incidentally, that from the purely materialistic point of view scientific research is not a luxury; for the community it is probably the cheapest possible form of investment. The Government's fuel research station has not yet proved the commercial possibility of the low-temperature treatment of coal which would result in the more economical production of smokeless fuel, oils, and gas; but in attempting this difficult task it has already, by results unforeseen when the task was undertaken, shown at any rate the possibility of economies for the State and for some of its major industries which are well in excess of the cost of the research itself.

There are parallels in many respects, as has been often pointed out and as often forgotten, between the periods of our history following the Napoleonic Wars and the Great War. The application of science in industry and daily life received impetus in the earlier of these periods in such directions as the introduction of steam motive power; it is receiving it now, as it has been attempted here to show. The auspices now are more favourable. Science is more powerful. Men more adequately and more generally recognise its power, and therein should lie a certain ethical value for it as offering a new point of view, in the manifold interest of which all can share. Should not the application of science, for example, offer a new field for community of interest, not only between one industrial organisation and another, but also within the whole body of workers in any single organisation?

In order, however, that the community may fully realise all that it owes, and all that it might owe, to the advancement of science, the channels of communication between research and the public mind have to be kept clear, maintained and widened. The non-scientific public is accustomed to view science as it might view a volcano; prepared for the eruption of some new discovery from time to time, but accepting the effects of the eruption without realising the processes which led up to it during the preceding period of quiescence. The period of preparation by research before science can offer the world some new benefit may be long, but the scientific machine is always running quietly in the laboratory. There is an example ready to our hands. We recall the introduction of wireless telegraphy and telephony as a scientific gift of quite recent years. Do we all realise that it was here in Oxford, at the meeting of the British Association so long ago as 1894, that the first public demonstration of wireless signalling by means of electro-magnetic waves was given by Sir Oliver Lodge? It was the work of science to develop the methods then demonstrated until they have been brought to their present marvellous uses. On the other hand, it is often the case, whether in industrial or agricultural, domestic or whatever application, that science has knowledge at command, awaiting use, long before mankind can be brought actually to apply it. Though we have quickened, we are not yet so quick in the uptake of the results of applied scientific research as, for example, some of our commercial competitors. The public support of scientific research, upon all these grounds, should be accorded freely, with understanding, and with patience.

Views and Reviews.

Summaries of Addresses of Presidents of Sections.[1]

The Analysis of Line Spectra.

In his presidential address to Section A (Mathematics and Physics), Prof. A. Fowler refers especially to recent developments in the analysis of the more complicated types of line spectra. The way to the analysis of such spectra was opened up by Catalán's discovery, in 1922, that in the spectrum of manganese and certain other elements there are terms of higher multiplicity than the triple terms previously recognised. Systems of terms with multiplicities up to octets have been found, and the main features of the structures of the spectra of a large number of elements have been worked out by Catalán, Meggers, and others. No exceptions to the law of alternation of even and odd multiplicities in passing from element to element across the periodic table have been found, and the first spark spectrum of an element has the same even or odd multiplicity as the arc spectrum of the preceding element. The types of the spectroscopic 'ground terms,' representing the deepest energy levels of the respective atoms, have also been identified for numerous elements, though their actual values are not always determinable.

The more recent results of the analysis of complex spectra have thus provided an ordered knowledge of a multitude of facts which have an important bearing upon the theory of spectra and on the arrangement of electrons in the outer parts of atoms. Theoretical workers have kept pace with the experimental progress, and the deeper spectroscopic terms can be calculated for any specified electron arrangement by means of formulæ developed by Hund. The scheme of electron distribution suggested independently by Main Smith and Stoner can now be applied with considerable confidence to all the elements from 1 to 92, and the theoretical predictions as to the characteristics of the spectra will doubtless be of great assistance in the analysis of spectra which have not yet been disentangled.

Reference is made to some of the experimental methods of spectroscopy, from which it would appear that the present resources are adequate for the elucidation of the majority of the outstanding problems.

The Scope of Organic Chemistry.

Prof. Jocelyn Thorpe discusses the field which lies before the organic chemist in his presidential address to Section B (Chemistry). During the fifty years that have elapsed since van 't Hof published the theory on which modern structural organic chemistry is based, great and important advances have been made in our knowledge of the chemistry of carbon. The tetrahedral theory of carbon structure provided a means of attack which has enabled the organic chemist not only to gain an insight into the conditions underlying the formation of simple molecules, but also has rendered it possible for him to prepare synthetically many of the products formed during life processes. Nevertheless, he is still far from gaining a knowledge of the mechanism underlying Nature's chemistry. The van 't Hof theory has served the chemist well and has carried him far, but he is now seeking another lead to carry him still further forward.

Structural organic chemistry must conform to the modern views of the physicist regarding molecular structure, and it is clear that some form of the electronic hypothesis will have to be adopted; but at present, although there are many such hypotheses, they all have one feature in common—they are qualitative not quantitative—they explain but they do not predict. In this respect, therefore, they differ fundamentally from the great conceptions of Dalton and of van 't Hof.

The scope of organic chemistry includes biochemistry, but the modern tendency is to approach biochemical problems from the biological side rather than from the point of view of structure. If biochemistry is to justify its name, greater efforts will have to be made to develop the science from the organic chemical side, otherwise any reasoned system of advance will be impossible. Our forest products need far more attention. Originally most of the useful organic products were derived from materials obtained from the plants and grasses present in our forests. By means of these substances it was found possible to correlate structure with physiological action and to produce compounds of enhanced value synthetically. It is certain that many such substances still remain to be discovered.

The scope of organic chemistry includes also the dyestuffs and petroleum products. The former constitutes probably the most remarkable example of the application of the van 't Hof theory to scientific and ultimately to commercial needs, and teaches a lesson which no one can afford to ignore. Great and successful efforts have been made during the past ten years to render Great Britain self-contained as a dyestuff producing centre, but the need for fundamental research must not be overlooked. Petroleum stands sadly in need of scientific treatment if the wasteful and uneconomical processes engendered by the abundance of this material are to be replaced by the economical and frugal methods which can only be determined by systematic scientific research. Moreover, the modern tendency to construct engines of increased compression ratio means that there will be an increasing demand for the higher boiling hydrocarbons as motor fuel. Polymerisation and depolymerisation must therefore be made the subjects of systematic investigation.

The Lower Carboniferous (Avonian) Rocks of England and Wales.

After reference to the death at Oxford nearly ten years ago of that brilliant worker on the Lower Carboniferous rocks, Arthur Vaughan, Prof. S. H. Reynolds in his presidential address to Section C (Geology) summarises the progress of knowledge on a variety of geological topics, which are unrelated, except in so far as they are concerned with the study of the Avonian.

The subdivisions of the Avonian rocks are first

[1] The collected presidential addresses delivered at the meeting are published under the title "The Advancement of Science; 1926," at 6s., or obtainable at the bookstall at Oxford by members at 4s. 6d.

discussed and Vaughan's earlier and later views are indicated. There has been considerable increase in our knowledge of the unconformities and breaks in the succession of the Carboniferous rocks, and the phasal equivalents were dealt with in some detail; suggestions are made in the hope of lessening the confusion which has arisen from the inconsistent use of certain symbols. Though the Millstone Grit lies outside the scope of the address, its relations to the underlying strata are described, and the varying relationships to the rocks below of the Millstone Grit (*sensu lato*) in the Bristol district, South Wales, and the Yorkshire Dale country respectively were indicated.

The problem of the classification and nomenclature of the Dibunophyllum zone (*sensu lato*) and its equivalents in the north of England is a controversial one. The goniatite work of Mr. W. S. Bisat has, however, led to the rocks of the Culm or shaly phase being known with an accuracy which is sometimes lacking in areas where the rocks are of the calcareous type.

Our knowledge of reef-knolls, pseudobreccias, and algal limestones is summarised, and the address concludes with sections devoted to dolomitisation and chert.

Biology and the Training of the Citizen.

Prof. Graham Kerr, in his presidential address to Section D (Zoology), emphasises the condition of dangerous instability occasioned through the evolution of the modern civilised State not having been accompanied by a corresponding evolution in the training of its citizens. One of the chief dangers lurks, curiously, in what was one of the most important factors of human evolutionary progress, namely, the development of language, for this, through modern applications of science such as printing and radio telephony, has come to interfere with the natural working of the principle of leadership—upon which human progress is so dependent. The natural leaders in communal advance throughout the ages have been the men of outstanding constructive ability; leadership has now, however, been made accessible to those whose ability is not constructive but is rather that of the advocate.

The great dangers of the position so arising can only be avoided by immunising the citizens by means of education, and Prof. Kerr urges the importance of including a grounding in the main principles of biology in the reformed educational system. A grasp of such main principles as evolution, inheritance—including the fact of its incompleteness, *i.e.* what is ordinarily called variation—the struggle for existence, communal evolution, and the inter-communal struggle, would, by inducing in the citizen's mind the biological outlook, broaden his vision and enable him to see his way round dangerous obstacles in the path of social advance. He would appreciate the biological aspect of economics, and, realising that the wealth of the community lies in the capacity of its citizens, would recognise how greatly it is to the commercial interest to increase that capacity by the training of the individual. Mass production in industry depends for its success on uniformity of raw material; such uniformity is absent in the human raw material, and therefore the principle of mass production should, as regards the education of the citizen, be confined within the narrowest limits consistent with practical considerations. With the limitation of mass education would come a quickening into renewed life of the primitive relations of parent and offspring—the deadening of which is one of the deplorable phenomena of existing conditions. The quickened interest in the training of his individual child would stimulate the interest of the parent in our whole system of education—the lack of such interest being again one of the great weaknesses of our present-day democracy.

It is a necessary feature of advance in civilisation that the individual citizen, his abilities, mental and physical, no longer kept up to the highest pitch by the pressure of the struggle to exist, tends to degenerate and depend to a greater and greater extent upon the community. This increased dependence carries with it increased obligations of an ethical kind towards the community—discipline, self-sacrifice, patriotism, loyalty. The shackling of personal liberty in the bonds of duty to the community would again be a return to ancient custom dictated by biological principles.

In conclusion, Prof. Kerr urges that the biological view of society which, while recognising differences in detail, whether in ability or in manners, never loses sight of the fact that behind these lies a human nature fitted out with the same common strengths and trammelled by the same common weaknesses, greatly conduces to the smooth working and onward progress of the social mechanism.

The Economic Development of Tropical Africa.

The task of developing Africa is complicated, as is shown by the Hon. W. Ormsby-Gore in his presidential address to Section E (Geography), by two factors, climate and the wide differences in traditions and capacities between the white ruling race and the native population. As a result of climate manual labour cannot be undertaken by Europeans. The African is the most adaptable of all the races of the human family; but the changes which have been produced so far are not more than skin deep. We know little of the history and mental traditions and aptitudes of the African native. Year by year anthropology is adding to our knowledge, but we are still at the stage of collecting data, and it is difficult to ensure that scientific investigation shall keep pace with the practical day-to-day running of Government administration and economic development. Nevertheless, we must clear our ideas on certain fundamental questions.

The first problem is the health of the African population. It is difficult to say with certainty what is the effect of European impact on vital statistics. In tropical Africa, though there are no scientific data, there can be little doubt that the population diminished between 1900 and 1924. This is due mainly to sleeping sickness and venereal disease; but the principal cause is infant mortality, which under the age of twelve months in the purely native area of Tanganyika has been estimated to be so high as 400 per 1000.

Rapid economic development has given rise to a number of social problems as a result of the accession of wealth among native producers. It has affected the notions of land tenure, and the individual has become emancipated from communal control. The nature and sanctions of tribal authority are undergoing rapid change, especially spiritual and moral. In the coast

towns a large portion of the native populations has become entirely detribalised. They are imitating Western civilisation and demanding Western education; but in education the real needs of the people and the right methods of bringing out their innate capacity on modern scientific lines have not been studied. The disrupting tendencies in Africa to-day are so many that constructive thinking is urgently necessary, if African communities are not to be reduced to disorganised mobs. The introduction of new arts and industries has created a class of wage earners unknown in the old Africa; but the incentives to labour are small. There is danger of degeneration unless the young men are trained to work either as direct producers or wage labourers.

There are four main duties upon which we have to concentrate. First, the various problems of 'public health'; second, the improvement of the standard of the natives as producers of food and economic crops; third, to further transport facilities; fourth, to educate the native in such a way that he may advance in the scale of civilisation and assimilate such new moral controls as will fit him to withstand the dangers and make the best use of increasing wealth.

Inheritance as an Economic Factor.

The purpose of Sir Josiah Stamp's address to Section F (Economic Science and Statistics) is to consider the whole subject of inheritance in its relations to economic science and the community. The conclusions reached are as follows:

1. In the past century unprecedented economic advance has been due in the main to the greater use of invention and fixed capital. This has, in turn, made new accumulation of savings possible, and has been made possible by the growing fund of accumulation. In this accumulation the principle of inheritance or bequest has played an important part. Where there has been freedom from the shackles of a family diffusion system the greater progress has been possible. The individual motives which are operative under such a system are stronger than ever, but operate over a diminishing part of the field; they are also stronger over a short period, and of diminishing effect over a long period of time. In other words, communal saving *via* company reserves (not subjected to the individual volition for saving against spending), and *via* repayment of debt through funds derived from taxation, and *via* large capital efforts (housing, etc.) partly financed through taxation, is an increasing proportion of the total. Although some of the values set up by such collective sums may figure in individual estate values, they are not created or destroyed by interference with, or promotion of, the right of inheritance.

2. The remaining considerable section of capital accumulation is still powerfully affected by inheritance rights, and would be more affected than heretofore by interference with rights in the direct line, though less affected than hitherto by rights out of that line.

3. The sense of 'social injustice' is directed against inequality of wealth, of which inequality through inheritance is not now the larger part. This sense, if limited to inheritance features, has less economic reaction than is generally supposed. In any case, it is a sense which is not scientifically based. It is probable that the average man has a slightly smaller *proportionate* share of the aggregate than he would have had if there had been no inheritance system, but a substantially larger *absolute* amount, because he shares a larger aggregate or better standard of life than he would have had under a system with no such aid to accumulation.

4. The particular claims for unlimited rights of bequest, as settling the best economic direction and control, are gradually losing their force.

5. The principles upon which death duty taxation is at present based might be improved. The actual sum now being raised is not necessarily more harmful economically than a similar sum raised by additional income-tax, but it is more repressive in accumulation than the same sum would be if a less sum were raised at lower rates on the first succession, and the balance were raised at higher rates on succeeding successions.

Position and Prospects of Electricity Supply.

Sir John Snell, the president of Section G (Engineering), is giving an address on the present and future development of electricity supply. His predictions about the future are based on the latest and most accurate data, and they summarise the results of many lengthy and laborious calculations. Sir John gives first a historical résumé of the progress that has been made during the past forty years. He lays special stress on the work done by Ferranti and Crompton. It is interesting to remember that although Sir Charles Parsons perfected his steam turbine only about thirty-five years ago, yet of the total prime movers now used in electrical generating stations, less than 10 per cent. are reciprocating engines. He points out that the average load factor, that is, the ratio of the actual power delivered to the maximum possible power, is, in Great Britain, less than 25 per cent. Hence the capital expenditure is four times as great as it would be if the plant worked continuously at full load. For economy, therefore, it is of vital importance to have a high load factor. Stress is laid on the importance of having common sources of supply for power, light, and railway traction. In this way a high load factor can be obtained.

The present output of the public generating stations in Great Britain is 7000 million units annually. Government publications estimate that in fifteen years the output will be three times as great. This is a low estimate, as no account is taken of the great probable future extension of railway electrification. The electricity returns record an average rate of increase in the sale of electricity between 1913 and 1924 of 25·8 per cent. per annum. If this continue, the progress of the industry will be extremely rapid.

It is necessary, therefore, to take long-sighted views of the future. In an industrial country like Britain, in order to live and continue to exist as a great nation, it is essential that abundant power be available. The bulk of this power must be derived from coal. In Scotland and North Wales large hydro-electric systems are in operation. In the Grampians, in Ayrshire, and in Lanarkshire water-power systems are being erected. Detailed investigation is being made as to the tidal power available in the Severn estuary. It is estimated that a 1000 million units of electricity could be distributed from this source alone. Compared with other

countries, however, Britain is poorly endowed with water power.

The importance of economising the consumption of coal is emphasised. Some of the devices suggested only economise fuel at the expense of undue capital expenditure. Increasing the size of the plant and employing bigger stations will undoubtedly economise coal. Sir Charles Parsons has shown that with a steam pressure of 500 lb. per square inch and a 97·5 per cent. vacuum, a full load thermal efficiency (steam to electricity) of 33½ per cent. is available. With an initial steam pressure of 1000 lb. per square inch, a thermal efficiency of 35 per cent. can be obtained. It is probable that in a few years electricity will be generated at large stations at a cost not exceeding 0·3*d*. per unit. This would mean that if the price of coal be about 20*s*. per ton, the *average* price paid by a consumer would, in a few years' time, be about 2*d*. per unit for lighting and 1*d*. per unit for power. Some consumers in remote districts will have to pay more and others less than this. Although a steady development is discernible, it is certain that there is a great future before electricity supply. It is our duty to lay down suitable lines along which the industry may develop so that succeeding generations may reap a bountiful harvest from our foresight.

The Regional Balance of Racial Evolution.

Prof. H. J. Fleure in his address to Section H (Anthropology) tries to interpret the present-day distributions of physical characters of man on evolutionary lines rather than on the more purely taxonomic lines that have been generally recognised in earlier studies. The general evolution of man is touched only lightly to suggest relations between lengthening of prenatal life on one hand and increased brain growth and decreased hair and jaw development on the other. Attention is concentrated upon men of modern type, known from the European Aurignacian period onwards, and comparative survey suggests that certain skull changes accompanied the evolution of modern types, that evolution probably occurring over a broad zone from Cap Verde to Persia. The arguments for this area are partly palæoclimatic. Early drifts southward from this early home are held to have given rise ultimately to the pigmy peoples (heads not lengthened), Bushmen and Tasmanians (heads often lengthened), Australians (heads much lengthened), with diversities of colour and hair. It is suggested that the people on the northern fringe of the early home, like some on the southern fringe, may have escaped the head lengthening seen in others, and that from these, better grown and less dark than those on the south, the sub-brachycephalic and ultimately the brachycephalic peoples of the world's highland zone may have originated.

The way northward between the Elburz Mountains and the Hindu Kush is held to have been opened up to modern men with the final retreat of the ice, and drifts through it went mainly northeastwards leaving the high Mongolian plateau and Tibet on the right. The extreme conditions of the high Mongolian plateau in their probable relation to racial evolution and drifts into China, etc., are mentioned, and the question of race types in Europe is argued out on similar principles. It is suggested that evolutionary change owes a great deal to cumulative changes of environmental influences in plastic infancy. As, however, no two embryos are exactly alike, the hereditary units in some may vary towards, in others away from, the new standards arising from the cumulative changes of environmental factors and as a consequence health and growth and survival may be affected. In other words, 'selected' germinal variations may follow after somatic variations and may thus help to secure the newly gained features to the race.

Function and Design.

Prof. J. B. Leathes' presidential address to Section I (Physiology) deals with the interesting subject of function and structure in the living organism. The parts of which a living organism is composed assist in the maintenance of its life. For physiology, which studies the manner of their ministrations, their morphological structure is organic design. This structure, essentially chemical, serving in the maintenance of life, must always display the peculiar property distinctive of matter in which the phenomena of life are exhibited, that of transforming adjacent matter which is not alive into its own similitude and so, perishable though it be, maintaining itself by spontaneous self-regeneration. A chemical structure so designed as to perpetuate a perishing existence is a necessary hypothesis for biology.

Twentieth-century chemistry, which already, by defining the catalytic nature of enzymes and the general nature of the material of life, has shown the immensity of the field to be explored, may yet also dispel some of the obscurity that covers it by indicating structures that have in some degree the property which the hypothesis requires, structures that assimilate the disposition of adjacent foreign matter to their own, and so in spite of their instability tend to perpetuate themselves. Of all degrees and qualifications that chemistry can put after or before its name, 'biological' will then be that of which it will have most occasion to be proud.

Living material constituted so as to behave in this way appears to be largely, though certainly not entirely, protein in character. The almost infinite number of forms that protein substances may take, even without changing the proportions in which the amino acids of which they are composed occur in them, fits with the variability of forms of living matter. If some of these variations, retaining the power of self-regeneration, are sensitive to recurring external disturbances, and react to them in such a way as to favour the manifestation of this power, these variations are more likely to survive.

The evolutional history of life must express the behaviour of the chemical constituents of the material that lives, the distinctive property of which is that of spontaneous self-regeneration. A modification of living matter which retains this property, if it assists in the maintenance of life, must persist; function alone can give permanence to structure. Modifications without organic design, that obstruct or merely do not assist, must share in the decay, but cannot share in the reconstruction of living matter.

Psychological Aspects of our Penal System.

Dr. J. Drever points out in his address to Section J (Psychology) that the evolution of punishment and

a penal system exhibits four phases, which may be designated the vindictive, the retributive, the protective or deterrent, and the reformatory. The vindictive phase is not punishment in any strict sense. Nevertheless, it represents the psychological origin of punishment. An individual who has suffered at the hands of another responds with the impulse of anger, which is satisfied by the infliction of some hurt on the culprit. The hurt inflicted may bear no direct relation, either in kind or in degree, to the injury done, but merely to the intensity of the anger. When some crude notion of justice comes into existence and determines the kind of hurt that may be inflicted on the wrongdoer, the vindictive passes into the retributive phase. This in turn passes into the deterrent phase, when it is realised that punishment looks to the future as well as the past, and that its function, from the point of view of society at least, is to protect against the repetition of a delinquency. As civilisation advanced it began also to be realised, at least in theory, that no amount of wrongdoing on the part of the delinquent absolves society from responsibility for the results produced on him and on others by the way in which he is treated by its representatives, acting under social laws. The realisation of this truth, and the perception at the same time of the fact that one way in which society may protect itself against wrongdoing is by the reform of the delinquent, marks the transition to the reformatory phase.

At the present time it will be generally agreed that, so far as society uses punishment at all, it ought to do so with some definite end in view, and therefore with an eye to the future rather than the past. It would also be generally agreed that this end is primarily its own protection, and, as subsidiary to this, the producing of such a psychological change in the delinquent that the wrongdoing will not be repeated. Some writers have suggested that we ought to regard the action taken by society as treatment rather than punishment, the suggestion involving the view that delinquency should be considered as the outcome of something not unlike disease. There is, however, really no warrant for excluding either the idea or the fact of punishment, which exercises an important influence in the direction of reform in various ways. At the same time, it is true that if, and as, society progresses, we may expect the need for special emphasis on the deterrent influence of punishment gradually to diminish, and the reformatory aim to come more and more into the foreground.

If punishment looks to the future rather than to the past, it is evident that the vitally important psychological problems of punishment are not those involved in the question of criminal responsibility, which is really a question inherited from an outworn penal philosophy, and not, from the social point of view, an urgent practical problem at all. The important psychological problems are rather those involved in a thorough knowledge of the psychological situation with which we are faced in dealing with the individual delinquent, and of the psychological effects likely to be produced by the action taken in the name of society. The first step in penal reform at the present time should be to provide for the securing of such knowledge by the institution of psychological clinics for the examination of delinquents, and more particularly juvenile delinquents.

Changing Aspects of Botanical Science.

Prof. F. O. Bower, in his presidential address to Section K (Botany), refers first to the calamity which has fallen upon the Section, in the sudden death of the president-elect, Dr. William Bateson. Instead of attempting to fill the broad biological rôle which would have been natural to Bateson, Prof. Bower centres his address round the dates of the previous meetings in Oxford. After a brief reference to the stormy meeting of 1860, he points out from recent writings how Darwin's views, as stated in "The Origin of Species," still stand in essentials as a great philosophical pronouncement. He directs particular attention, however, to Ray Lankester's introduction of the conception of homoplasy, which followed as a natural sequel to it. This was applied first in animal morphology, but Prof. Bower illustrates it by reference to homoplastic likeness even in internal tissues of plants, an aspect that is still in its infancy.

Passing to 1894, the central feature of that meeting was Strasburger's enunciation of his generalisation on the periodic reduction of chromosomes. It fell like a bomb-shell upon the old controversy on alternation in plants. But it was soon recognised that here was something more fundamental than somatic alternation. Like the fabulous genie let loose from its bottle, the conception of a nuclear cycle in all plants that show sexuality dominates ever more and more the field of morphology. That cycle may be regarded as the foundation upon which somatic development may or may not be imposed. Provided the events of syngamy and meiosis be constant, the alternating generations must have always been distinct: if not, then there is a possibility of alternation not by 'interpolation' but by 'transformation.' The resolution of this problem can be given only by close comparison of facts derived from nearly related organisms. But until this final resolution comes, it is possible to hold either a theory of 'transformation' or of 'interpolation.' Prof. Bower still thinks that the greater probability appears to lie with the latter, as regards green archegoniate plants. It is open to anybody to see in the Archegoniatæ a separate evolutionary problem from that of any of the great lines of Algæ. Wide homoplasy may be invoked, and an origin of the sporophyte in land plants contemplated by interpolation. The discovery of the Rhynie fossils has strengthened this view by providing early synthetic links between Bryophytes and Pteridophytes, and by thus drawing the Archegoniatæ more definitely together.

As to the botany of the present period in Britain, its relation to previous periods can best be traced through the pages of the *Annals of Botany*—that stately series of forty volumes which have since August 1887 emanated from the Oxford Press. The first volumes bear evidence of a preponderance of organographic and systematic interest. Then anatomy and cytology followed in increasing proportion, and later there has been a preponderance of memoirs relating to physiology. The drift of research has clearly been from systematic and descriptive science to a prevalent study of function. Doubtless time will right the balance of opinion again: but meanwhile a cool philosophical outlook will estimate no one branch of the science as of greater importance than another.

Man and Method in Scientific Education.

Sir Thomas Holland's presidential address to Section L (Educational Science) discusses two questions relating to scientific education: first, the best arrangement of curricula and examinations, and secondly, the cultural value of education in scientific subjects. Under the Royal College of Science system, to which the name 'tandem' system has been given, one subject is studied at a time, two subjects for half a year in each academic year during the first two years of the course, and one subject at an advanced stage during the third year. Examinations are held at the end of each course and closely follow the teaching. The alternative system, based on the University of London examinations, involves simultaneous study of three or four subjects during the first year, leading to the Intermediate Examination, followed in the case of pass students in science by two years of simultaneous study of three subjects, or, in the case of honours students, to the intensive study of a principal and a subsidiary subject.

Sir Thomas Holland's favourable opinion of the 'tandem' system is confirmed by the opinion of many professors who have had experience of the system in the first instance as teachers. He recommends it especially for students who are to devote their lives to scientific research. "Our professors," he says, "still adhere to the tandem principle, although one detects in the time-tables a slight yielding to the demands of the more complex life; but an extension of the course from three to four years in most of the 'schools' has helped still to preserve the simplicity of the students' night thoughts." It is not easy to quote the merits of the two systems. For those who are to find a career in business or administration in which many unrelated questions will have to be handled daily and with promptness, mental mobility may be increased, or, at any rate, more rigidly tested, by the composite system of training and examination. The 'tandem' system, as carried on at the Royal College of Science, has been formally approved by the University of London. Whatever other merits it may have, it has induced the University to relax its cast-iron regulations. Sir Thomas Holland points out that the 'tandem' system is applicable not only to different subjects, but also to different sections of the same subject, such as the petrological and palæontological groups in geology.

The remainder of Sir Thomas Holland's address is devoted to the question of 'humanising' scientific education. Are we not inclined to forget that the Renaissance, the great world-revolt from formalism and theological bondage, was a revival not only of literature and art but also of 'the spirit of research' regarding natural laws? In course of time, classical learning tended towards specialisation in its linguistic, grammatical, and rhetorical aspects. "Its main object became obscured and stricken with a formalism and even pedantry." Nevertheless, Sir Thomas pays generous tribute to the classical curriculum in which our chief public men and our army of administrators, at home and overseas, who have made the British Empire what it is, have been nurtured; and insists that while in scientific education attention must be given to the professional and vocational aspects, we must see to it that science and technology are taught in a way which will develop character and capacity for affairs.

Food Supply and Population.

In 1898 Sir William Crookes directed attention to an approaching scarcity of wheat in the world owing to the exhaustion of the nitrogen in the soil. Crookes's prophecy may seem to have been wholly falsified, but none the less we cannot continue to ignore Malthus's conclusion that the food supply of the world must eventually begin to lag behind the increase of population. Mr. Keynes has given reasons for the conclusion that the course of wheat prices and real wages since the War already indicates the approach of scarcity, and though Sir William Beveridge has rejoined that the limits of agricultural expansion are still remote, it is desirable to examine the fundamental relation between land cultivated and population maintained. This is done by Sir Daniel Hall in his presidential address to Section M (Agriculture).

By various means of approach it can be shown that, under existing conditions of farming, about 2½ acres of cultivated land are employed for the maintenance of one unit of population—man, woman or child. This area varies in different countries, according to the quality of the land and the system of agriculture followed. It may be so low as 1⅘ acres in Denmark with its intensive arable farming: so high as 4 acres in Spain with great areas of poor pasturage.

In the ordinary way, however, expansion of the population demands a corresponding increase in the cultivated area, and the gigantic increase in the world's population during the nineteenth century was the direct outcome of the settlement of the Americas, Australia, and kindred 'new countries.'

Reasons can be given for supposing that an expansion of the area corresponding to the present rate of increase in the white population cannot be continued. Suitable land is no longer available in quantity commensurate with the demand. It remains to increase the production from the existing farmed area, and though possibilities in this direction are enormous, because, for example, the great wheat-producing areas of the world are farmed at a very low level of yield per acre, such increase of production will involve higher cost of production per unit. The only means of escaping from higher costs lie in the applications of science, such as the improvements due to the plant breeder, or the cheapening of fertilisers brought about by the manufacture of synthetic nitrogen products. The low returns derived from agriculture at the present time militate, however, against intensification of production; indeed, in most civilised countries a 'flight from the land' of both men and capital is manifest because its earning capacity is low compared with that derived from industry and commerce. Intensification of production is only likely to begin when stimulated by a definite rise in the prices of agricultural produce, but this must occur as the land available for settlement proves insufficient to absorb the present surge of population before the checks that are in action prove effective.

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Our Bookshelf.

Ability and Behaviour.

Ability: a Psychological Study. By Victoria Hazlitt. Pp. ix+147+2 plates. (London: Methuen and Co., Ltd., 1926.) 6s. net.

PART 2 records three years' work on "tests for special abilities in the work for Arts and Science degrees." Part 1, the result of the author's consideration of current theories of 'intelligence,' capacity and ability in the light of this work, is a capable analytic treatment of these theories coupled with an interesting new synthesis. General capacity is inborn: special abilities are developed. "The original endowment may be used in a number of ways. However it is used, it will lead to the development of special abilities" (p. 74). Prof. Spearman's 'engines' are not innate but acquired.

The construction of tests is easier in science than in arts. This is due to two causes. First, the practical work necessary in all sciences constitutes common ground which is readily tested. Secondly, special arts abilities are intrinsically more difficult to test because "the material of the natural sciences is less complex than that of the humanities" (p. 144). Still, three tests, successful in diagnosing arts ability, have been found, and results obtained support "the hypothesis that good general intelligence is more necessary for work in the Arts faculty than for work in the Science faculty" (p. 144). R. J. B.

Brains of Rats and Men: a Survey of the Origin and Biological Significance of the Cerebral Cortex. By Prof. C. Judson Herrick. Pp. xiii+382. (Chicago, Ill.: University of Chicago Press; London: Cambridge University Press, 1926.) 15s. net.

IN this book Prof. Herrick attempts to interpret the behaviour of rats and men in terms of cerebral anatomy. In the present state of our knowledge, any satisfactory solution of this interesting but immensely difficult problem is obviously unattainable. Nevertheless, it is a fascinating occupation to wander in the borderland between brain and mind and collect stray ideas as to possible points of contact.

The book may be of value to the student of psychology and the general reader. For those who are ignorant of such matters it provides an easily understood introduction to certain aspects of the comparative anatomy of the brain, in particular the cerebral cortex, and the results of an interesting series of experiments on the behaviour of rats, with tentative suggestions as to how and why some of the activities of the brain should express themselves as mental phenomena.

The book is, however, too elusive to be of much use to the serious student. This is due not solely to the fact that the author overuses vague phrases until they become little more than meaningless catchwords, but even more to his neglect of the fundamental conceptions of neural physiology that are associated with the names of Hughlings Jackson, Sherrington, Head, and Magnus, among many others, which represent the essential principles for the interpretation of animal behaviour. The inhibitory influence of the cerebral cortex and the phenomena due to the release of its control, the regulation of posture and the amazing perfection of the automatic mechanisms for controlling the attitude of the body, and in particular the conception of a hierarchy of neural levels upon each of which a different kind of functional result is achieved—these are all matters that are of primary importance for the interpretation of animal behaviour in neurological terms, which are not given due consideration in this book. In particular, however, the chief criticism of Prof. Herrick's fascinating attempt to give a biological explanation of mental phenomena is his failure, when using his terminology, to appreciate the full significance of Sir Charles Sherrington's contribution to the general fund of knowledge of such matters.

Manners and Customs.

Le monde islamique. Par Max Meyerhof. (Bibliothèque générale illustrée, No. 3.) Pp. 80+59 planches. (Paris: F. Rieder et Cie, 1926.) 15 francs.

THIS little book aims at extending among the general public a knowledge of the origin, the character, and the extent of Islam—a civilisation which, as the author says, is composed of the most heterogeneous elements, and yet is not without a certain unity. To this unity has been added since the War an element of hostility towards European control and intervention which is very real, even though its effect as a consolidating force may have been exaggerated in some degree. M. Meyerhof's treatment has necessarily been very summary, and of his sixty-eight pages, nearly half is devoted to an analysis of the recent course of political events. In these, pan-Islamism and the racial movement tend to obscure undercurrents of opinion which have been potent forces in counteracting the movement towards unity. Admirable as is M. Meyerhof's sketch of Moslem religion and culture within the very restricted limit imposed by the nature of the popular series of which his book forms a part, it would have gained still further as a picture of actual conditions had he given a more systematic account of the divisions in the Mohammedan faith which set one sect over against another. Apart from this, it is a clear and lucid introduction to an understanding of one of the grave world problems of the day.

The Southern New Hebrides: an Ethnological Record. By C. B. Humphreys. Pp. xvi+214. (Cambridge: At the University Press, 1926.) 12*s.* 6*d.* net.

ANTHROPOLOGISTS will be grateful to Mr. Humphreys for this ethnological record of the Southern New Hebrides for two reasons. Very little systematic work has been done on this interesting little group of five islands, if Speiser's work on the New Hebrides as a whole be excepted; and, as everywhere in the Pacific, changing conditions make it imperative that ethnological investigations should be pushed on before it is too late. Sophisticated as the natives of the New Hebrides are through their contact with Europeans, Mr. Humphreys was fortunate enough to find a few old men who still retained a memory of old customs. How important and how fortunate this was is attested by the intricate and somewhat puzzling character of the culture, especially on the island of Tanna. Mr. Humphreys puts forward a very interesting hypothesis to account for the differences in the form of the chieftainship in that island, the nebulous form of totemism found in the group, the variations in the cephalic index, and other physical characters as well as other peculiarities. It is difficult to suggest an alternative, and Mr. Humphreys' hypothesis must for the present hold the field; but it involves an exceptional history for the island of Tanna for which it is hard to account satisfactorily.

Natural History.

British Spiders: their Haunts and Habits. By Theodore H. Savory. Pp. xxi + 180. (Oxford: Clarendon Press; London: Oxford University Press, 1926.) 6*s.* net.

IN recent years, both in England and America, various attempts have been made by writers on the habits of spiders to direct the attention of the budding naturalist to a group of animals little studied, though everywhere abundant. But when interest has been aroused and the reader has been tempted to pursue the matter further and attain to a more intimate acquaintance with these curious creatures, he finds himself at a loss. The would-be student of butterflies or birds has his way made plain before him by a multitude of more or less satisfactory handbooks, but the books which deal in detail with the British spider fauna are either antiquated or unprocurable, or both.

Mr. Savory's book will help the aspiring araneologist at least one step on his way; and we have little doubt that it will be received with the hearty welcome it deserves.

Mr. Savory's main object being 'first aid' in the identification of specimens, he might perhaps have 'cut the cackle and come to the horses' a trifle more quickly. We by no means imply that the 'cackle' is not excellent, or that it is a mere imitation of previous prattlers—Mr. Savory is a keen observer on his own account—but, presumably, his greatest appeal will be to those who already know something of the biological side of the subject and are impatient to get to business. We heartily approve his plan of giving, where possible, the Blackwallian names of species, and thus rendering available Miss Staveley's book with its excellent reproductions of Blackwall's coloured figures—a book which he informs us is still obtainable. Savory plus Staveley should be a real help forward.

The illustrations of the book before us are its chief weakness. The figure of *Segestria senoculata* (Fig. 13) is really too crude, and Fig. 18 gives *Walcknæria acuminata* ten eyes! Then an accurate detailed drawing of the cheliceræ seems absolutely necessary. Without it, how is the student to interpret such terms as "primary fang teeth" and "lower margin of the cheliceræ" used in the dichotomic tables? We venture to direct attention to these slight blemishes in view of future editions, for which we anticipate a demand.

C. W.

Index Kewensis Plantarum Phanerogamarum. Supplementum Sextum: *Nomina et Synonyma Omnium Generum et Specierum ab initio anni MDCCCCXVI usque ad finem anni MDCCCCXX nonnulla etiam antea edita complectens.* Ductu et consilio A. W. Hill. Pp. iii + 222. (Oxford: Clarendon Press; London: Oxford University Press, 1926.) 70*s.* net.

THE appearance of a new supplement to the "Index Kewensis" is matter for rejoicing among botanical taxonomists, on whose behalf we thank the Director and the members of the Herbarium staff who have brought the record of names and synonyms of genera and species of flowering plants up to the end of 1920. The present supplement nominally deals with the years 1916–1920, but also includes many names unavoidably omitted from publications issued in 1914 and 1915, and also earlier names which have been traced since the publication of the original work and its successive supplements. One of the latter, "Stilla," W. Young (1783), followed by the remark 'nomen. Quid?' raises a question as to the desirability of indexing *nomina nuda*. Taxonomically they do not exist; they are neither 'names' nor 'synonyms,' and space would be saved by their omission. A certain French botanist is credited (?) with one or more in almost every other column.

It would be ungrateful, however, to cavil at so trivial a matter in a periodical of such interest; for the "Kew Index" makes as interesting reading as does a good dictionary; at any rate for the taxonomist who takes his work seriously. He may, for example, speculate as to what proportions of the entries in the columns represent genuine botanical work, and what are mere exercises in nomenclature; and as to whether the round-table conference between taxonomists at the recent Botanical Congress at Ithaca will prove a step forward towards the attainment of uniformity in nomenclature. The restoration of names which had been neglected or overlooked by earlier workers is responsible for a large number of entries, especially on behalf of American botanists; these are indicated by citation of the earlier name following the entry.

The "Index" records impartially foibles and follies and the results of honest work, and it brings to the notice of botanists rare and obscure publications. One of the latter (erroneously dated 1889) has yielded two and a half columns of names, credited almost entirely to one individual, under the genus Cinnamomum; a somewhat surprising entry is "*Crucifera*—genus omnia genera *Cruciferarum* amplectens"; while the immortality of a foolish action is illustrated

by the first item in the volume, "AA," under which genus several recent species are recorded. A. B. R.

Goldfish Culture for Amateurs: How to Breed and Rear Goldfish in Aquaria and Ponds. By A. E. Hodge and Arthur Derham. Pp. xi + 103 + 11 plates. (London: H. F. and G. Witherby, 1926.) 5*s.* net.

THIS small book describes a dozen or so of the varieties of goldfish and gives very clear and complete instructions for breeding and rearing them in aquaria. It deserves to be widely read, partly because the goldfish has in the past probably suffered more than any other domesticated creature from neglect, ignorance, and mistaken kindness, and partly because the authors show, in convincing fashion, that their cultivation is of great fascination. Moreover, considerable scientific interest attaches to an animal able to produce such remarkable mutations. This book may well inspire research into the origin and genetic relations of these varieties, with the possibility of results of unexpected value.

Birds in England: an Account of the State of our Bird-Life and a Criticism of Bird Protection. By E. M. Nicholson. Pp. xi + 324 + 8 plates. (London: Chapman and Hall, Ltd., 1926.) 12*s.* 6*d.* net.

UNDER the somewhat vague title of "Birds in England," Mr. E. M. Nicholson has given us a useful and stimulating book: its scope is better indicated by the sub-title, "An Account of the State of our Bird-Life and a Criticism of Bird Protection." The opening chapters discuss losses and gains in our avifauna and the operative causes. The middle portion of the book centres round an account of the lives and work of famous ornithologists. The final chapters deal with the practical problems of protection—sanctuaries, legislation, and the evils of collecting. The last part of the book, especially, is frankly polemic. The author holds decided personal views on questions of protection, and he presents these with force and skill. Not all his conclusions will be generally accepted, but his arguments always deserve serious consideration and often carry conviction.

Mr. Nicholson takes a rather unusual line, for example, on the subject of bird-sanctuaries, but the truth of many of his contentions must be admitted. He draws a useful distinction between sanctuaries in towns and those in solitary places. The object of the former is to attract birds where none lived before. The country sanctuary, on the other hand, is a retort to persecution, and the author regards it as in many ways an unfortunate one, tending to shelve the question of protection on a national scale. He points out how little can be done by scattered sanctuaries for the preservation of rare species other than those which breed in colonies, and he sees the true remedy in the effective prohibition of collecting. Nor does he omit to notice the unfortunate results which sometimes follow from indiscriminate protection.

From a purely scientific point of view, the earlier chapters, dealing with the "Balance of Birds," are the most valuable. These constitute a careful survey of the subject, from which much that is of interest emerges. In dealing with total gains and losses, Mr. Nicholson counts that fifteen breeding species have been lost, as such or altogether, since Tudor times, while seventeen have been gained: the figures include three which have been lost and regained and thus appear on both sides of the account. Of the gains, four are due to artificial introduction and the rest to natural colonisation, all within a century, by former purely visitant species, including seven ducks. The account of the decrease and increase of other birds, which have not been either totally lost or gained as breeding species, is perhaps even more interesting, and the thoughtful discussion of the probable parts played by different factors gives evidence of the author's thorough knowledge of the subject.

A Naturalist's Pilgrimage. By Richard Kearton. Pp. xiii + 246 + 8 plates. (London, New York, Toronto and Melbourne: Cassell and Co., Ltd., 1926.) 7*s.* 6*d.* net.

MR. KEARTON, by his photographs and lectures, has done much to popularise the simpler type of natural history, and he is justly proud of the achievement. This chatty autobiography traces his progress from the fell farm to the study and lecture hall, and with naïveté recounts the experiences, some instructive, many trivial, which he encountered by the way. The naturalist will be interested in the elaborate means sometimes adopted for the delusion of birds, and in the trying adventures of the pioneer days of bird photography; and if the impression is created that the author insists too much on his own cheerfulness and *bonhomie*, it is simply because he realises that here lies one of the secrets of his success.

Insect Studies.

The Fauna of British India, including Ceylon and Burma. Edited by Sir Arthur E. Shipley, assisted by Dr. Hugh Scott. (Published under the Authority of the Secretary of State for India in Council.) *Coleoptera: Chrysomelidæ* (*Chrysomelinæ and Halticinæ*). By S. Maulik. Pp. xiv + 442. (London: Taylor and Francis; Calcutta: Thacker, Spink and Co., Bombay: Thacker and Co., Ltd., 1926.) 25*s.*

THE family Chrysomelidæ, which comprises the leaf-beetles, is one of the largest in the animal kingdom. It includes to-day probably not far short of 20,000 described species which are grouped into sixteen subfamilies. The first eleven of the latter were dealt with in the "Fauna of British India" series in the volume by the late Mr. Martin Jacoby, published in 1908. In 1919 Mr. Maulik wrote the volume on the Hispinæ and Cassidinæ, while the Chrysomelinæ and Halticinæ form the subject of the present contribution. This leaves the Galerucinæ for future treatment, and, it may be added, they are exceedingly numerous in species. Of the subfamily Chrysomelinæ, only 63 species are recorded and are comprised in 18 genera. This paucity is remarkable, considering the richness of the faunal area surveyed, and it is evident that there are still far too few resident entomologists to do the necessary intensive collecting over so wide a region.

In the introduction to the Chrysomelinæ an account is given of the external morphology of the larva and imago, notes on the life-histories, and a list of injurious species found in various parts of the world. In the subfamily Halticinæ 287 species, in 70 genera, are recorded,

and there are similar introductory remarks on structure and habits. It is noteworthy that very many of the members of this subfamily are represented by single records only, and evidently these rather obscure beetles have afforded very little interest to collectors. In this volume, as elsewhere, Mr. Maulik has manufactured many new names from Sanskrit roots—a procedure which he says has saved him time which might have otherwise been used in discovering whether a name is preoccupied or not. The volume is clearly illustrated, although the sex of the examples figured might have been added with advantage. Its type and general arrangement are similar to those of its predecessors, and there appears to be an almost entire freedom from misprints. A. D. I.

Descriptions of new Genera and Species of Lepidoptera Phalænæ of the Subfamily Noctuinæ (Noctuidæ) in the British Museum (Natural History). By Sir George F. Hampson. Pp. iv + 641. (London: British Museum (Natural History), 1926.) 20*s*.

THIS volume contains the descriptions of more than 200 genera and a still larger number of new species of moths belonging to the subfamily Noctuinæ contained in the British Museum. Its MSS. was left by the author when he retired from the Natural History Department in 1920. Owing to the suspension of the Catalogue of Moths as the result of financial stringency occasioned by the War, a large number of species have remained in the Museum under Sir George Hampson's manuscript names without the necessary published descriptions. In order to obviate this disadvantage it has been deemed necessary to issue the present volume. Apart from bare descriptions, it contains no illustrations or other aid to the identification of this great mass of material.

Rural Science.

Insecticides, Fungicides and Weed Killers. By Dr. E. Bourcart. Translated from the French and adapted to British Standards and Practice. Second English edition, revised and enlarged by T. R. Burton. Pp. xii + 431. (London: Scott, Greenwood and Son, 1925.) 15*s*. net.

AGRICULTURISTS and horticulturists alike will welcome the appearance of a new edition of Bourcart's useful work of reference. During the fifteen years since the publication of the original edition of this volume, very considerable advances have been made in the application of chemical methods to agriculture. This has been specially marked in connexion with the suppression of plant pests and diseases, though much of the work is still in the experimental stage and needs applying with caution. In presenting a revised and enlarged English edition the translator has retained the fundamental basis of the original work, while replacing part of the old historical sections by details of modern methods and recipes which have been thoroughly tested and are worthy of recommendation.

Work on insecticides has proceeded rapidly in the United States, where it is aided and supervised by the Government, and in other countries also research is being actively carried on as the regions of cultivation extend, specially in the tropics and warmer temperate zones. This has greatly increased the possibility of dealing effectively with insect and fungus pests, and the volume under review aims at presenting the present state of our knowledge on the subject in so far as it has been sufficiently tested. For the sake of convenience, the entomological glossary, hitherto a separate section, is now incorporated in the body of the book. No bibliography is appended, probably on the score of space or unwieldiness, though recipes are in the majority of cases carefully attributed to their authors. W. E. B.

Rural Scotland during the War. By David T. Jones, Joseph F. Duncan, H. M. Conacher, W. R. Scott. With an Appendix by J. P. Day, and an Introduction by W. R. Scott. (Publications of the Carnegie Endowment for International Peace, Division of Economics and History: Economic and Social History of the World War, British Series.) Pp. xvi + 311. (London: Oxford University Press; New Haven, Conn.: Yale University Press, 1926.) 12*s*. 6*d*. net.

THIS valuable work embraces a complete survey of the economic and social history of Scotland during the War in its chapters on fisheries, agriculture with special reference to food production, the agricultural labourer and land settlement. There is also an appendix on the jute industry. No occupation suffered more severely, or was forced to undergo more drastic reorganisation, than fishing. The withdrawal of vessels and men, the closure of many fishing-grounds, and the shifting of the fishing ports from the east to the west coast, with the consequent difficulties of transport, are the chief problems discussed in Mr. Jones's survey. In agricultural Scotland, in spite of the heavy enlistments, there was an increased supply of food, especially as regards oats and sheep. Mr. Conacher deals fully with the problem the Scottish farmer successfully solved. Mr. Day's study of the jute industry traces the difficulties this industry had to face in its import of raw material, the rapid soaring of prices for jute goods, and the equally rapid post-War return to normal prices. The book is admirably arranged for easy reference and contains an abundance of statistical matters.

Cornish Geology.

Handbook of Cornish Geology. By E. H. Davison. Pp. 106. (Penzance: Royal Geological Society of Cornwall, 1926.) n.p.

MANY excellent books, pamphlets, and papers have been written about the rocks and geological problems of the Cornish peninsula. There are many unsolved riddles in this western land, so that there will always be a number of persons with some geological knowledge ready to turn a portion of their holidays to pleasurable account. The chief drawback of these excellent treatises is that they are too technical and obscure. They describe features in language which is not clear enough to the ordinary layman stranger, who requires to be directed to spots where these particular phenomena are to be seen at once with nothing to obscure the issue. Mr. Davison's book is perhaps scarcely free from the modern vice of multiplying technical terms

and names, which to the older geologist become confusing. The book supplies, however, a long-felt want. It brings the geological knowledge of the county up-to-date in very terse language, and it describes clearly and concisely the places and positions where the keys to various problems may be found. The chapters are short, but to the point; those on "The Granite," the "Lizard Rocks," and the "lodes of Cornwall" are especially good.

On such subjects as "the pneumatolysis [unpleasant word !] of the granite and other rocks" Mr. Davison is, of course, on controversial ground, but it is his own subject, and he steers clear of any points of fierce dispute in his own inimitable way. The chapter on "Geological Localities of Interest" is one which will be appreciated by those who have only a few days to spare in the Duchy. One feels that practically every spot worth visiting has been mentioned. Mr. Davison might perhaps have dealt a little more with the early Pre-Cambrian and Ordovician aspects of the region, in something of the style of Mr. Jukes Brown's excellent "Building of the British Isles"; but, no doubt, many of his readers will find the hints he gives in his little book a substantial basis for building further framework upon. The bibliography is very complete, but why did not Mr. Davison take it back a few years earlier? The period between 1890 and 1906 was a very prolific and useful one in Cornish geology. F. J. S.

Geophysics.

Was lehrt uns die Radioaktivität über die Geschichte der Erde? Von Prof. Dr. O. Hahn. Pp. v+64. (Berlin: Julius Springer, 1926.) 3 gold marks.

In this little book on radioactivity and geology, Prof. Hahn summarises in a form very convenient for German readers the outstanding results that have been achieved in recent years. Radioactivity is of fundamental importance to geologists in two main respects: the constant emission of heat, which is now held to control the surface history of the earth; and the accumulation of end-products in minerals, the analysis of which is making it possible to establish a geological time-scale and to correlate the Pre-Cambrian rocks.

Dealing with the age of the earth, Hahn discusses Joly's reasons for preferring lower estimates than those advocated by Holmes. He rejects the solvent-denudation method as being too uncertain to give trustworthy results, and suggests that the older geological estimates should be multiplied by ten or twenty. The results from thorium minerals are also rejected on account of the unsuitability of the minerals. These he regards either as secondary products or as leached residuals. The argument from pleochroic haloes is thought to be inconclusive, and the opinion is expressed that it is very improbable that uranium disintegrated more rapidly in past ages than now. No mention is made of the possibility of attributing the anomalies of the 'uranium' halo to the actinium series. It is clear that Hahn accepts the straightforward reading of uranium minerals, though it is unfortunate that he does not refer to the geological support given to the longer estimates by the remarkable work of Barrell ten years ago.

In his account of the earth's thermal state, Hahn reviews the work and opinions of Joly, Holmes, and Jeffreys (who, however, is not an American). He goes further than any of these writers in suggesting that life itself depends on the radioactivity of the rocks: without carbon dioxide, no life; without volcanoes, no carbon dioxide; without radio-elements, no volcanoes. A brief résumé is given of the reasons for supposing that uranium and thorium have been concentrated in the outer crust. The book concludes with a discussion of Joly's theory of basaltic cycles and the physical possibility of continental drift.

International Hydrographic Bureau Special Publication No. 12: Investigation of Harmonic Constants, Prediction of Tide and Current, and their Description by Means of these Constants. By Rear-Admiral Phaff. Pp. 80+6 plates. 5 Swiss francs. *Supplement to Special Publication No. 12: Tables for the Calculation of Tides by Means of Harmonic Constants.* Pp. 136. (Monaco: International Hydrographic Bureau, 1926.)

The needs of the seaman are not adequately met by tide tables for a limited number of ports; he cannot always deduce from them the best times for attempting to enter other ports, especially where there is large diurnal inequality in time and height of high water. The simplest solution of his problem is to compute by the harmonic method the tidal variation for a few hours of the required day. He should be provided with a list of the important constants for as many places as possible, with means to facilitate the computations, together with rules which cannot easily be misunderstood. These recent publications of the International Hydrographic Bureau profess to meet his needs; it is regrettable that they appear to have been thrust upon the world with little consideration and with great haste, even if we only judge by the liberal errata and corrigenda pasted in each volume.

The 'Tables' are for use with a method of computation based upon the work of Van de Stok; apart from an unsatisfactory method, the constants are not up-to-date, there is no value of mean sea-level and the phase-lags are given in degrees, local *or* zone time, without any indication as to which is used in any given case. The 'Tables' are not self-contained, lacking simple rules and typical forms for computation; the seaman, therefore, must first read the manual, and that will be fatal, for the language is frequently loose and obscure, and it is difficult to follow some of the processes (p. 53), even for one capable of independent reproduction of the results. The author gives formulæ and symbols without explanation (p. 59), and tables that are neither self-evident nor properly explained in the text (p. 57). In short, the manual appears to be transcribed from the author's notebook. It is incorrect to say that the constituents K_1, K_2 can be reciprocally eliminated in one day, whereas 365 days are required to eliminate K_1 from S_2; also, that 55 days are required to give a negligible contribution from O_1 in the analysis for N_2; less than 55 hours would suffice for that purpose. The objects are indeed laudable, but it is to be regretted that these volumes are unworthy of international acceptance.

Probleme der kosmischen Physik. Herausgegeben von Prof. Dr. Christian Jensen und Prof. Dr. Arnold Schwassmann. Band 7: *Der Massenaustausch in freier Luft und verwandte Erscheinungen.* Von Prof. Dr. Wilhelm Schmidt. Pp. viii+118. (Hamburg-Altrahlstedt: Henri Grand, 1925.) 6 gold marks.

THIS useful little book summarises the literature of the last fifteen years or so relating to transport phenomena in the atmosphere (and also, as a natural extension of the same ideas, similar phenomena in the ocean). The transport of heat and momentum and water vapour by atmospheric eddies is of great importance in meteorology, though the fact has been recognised only in recent years (largely through the work of Prof. G. I. Taylor). The author has himself written widely on these problems.

Modern Physics.

Reflections on the Structure of the Atom. By Florence Langworthy. Pp. xi+260. (London: Watts and Co., 1926.) 12*s.* 6*d.* net.

THIS volume, which covers a wide range, is concerned mainly with an atomic model in which the neutral atom of hydrogen is assumed to be built up of three "atoms of its isotope, ur-hydrogen," and three electrons. On this purely speculative foundation is raised an elaborate superstructure. "In my atom the double-positives (Alpha particles) are massed at the centre, and so I have the single positives free to unite with electrons and form planets that can be neutral, negative, or positive." The author is of opinion that a chemist will find the structure of her atom far more to his mind than that of the Bohr atom, which presents to the world outside negatives (electrons) only. The latter model she holds in scorn, going so far as to say that our knowledge of atomic structure "has been marking time all these years since the Rutherford atom became the Bohr atom":

In the new wave-theory of matter associated with the name of Schrödinger, a material particle is regarded as a singularity in a wave, but to the author of the present volume even the wave-theory of light is anathema. "Those who have handicapped their intellectual faculties by believing in 'waves' are now engulfed thereby." She claims to be one of the few who have always believed in Newton's corpuscular theory of light. Whatever may be the final solution of the problem of reconciling the quantum theory and the undulatory theory, it will not be reached by turning a blind eye to one side of the questions at issue. Truth is great, and will prevail; but in our view the cause of knowledge will not be advanced by the speculations in this book.

A Numerical Drill Book on Physics. By Prof. L. W. Taylor. Pp. viii + 95. (Boston, New York and London: Ginn and Co., 1926.) 1 dollar.

THIS collection of problems in physics, which should prove useful to teachers, presents some novel features of interest. Nearly every problem is first formulated in algebraic terms; four independent sets of numerical data are provided for each example, these being chosen so as to cause the results to pass through maxima and minima wherever possible; answers are given usually correct to four significant figures. The mathematical tables at the end of the book are clearly printed.

Chemistry and its Borders.

An Introduction to Chemistry. By C. G. Vernon. Pp. 276. (London: George Harrap and Co., 1926.) 4*s.* 6*d.*

MR. VERNON'S book is based on the application of the heuristic method. It proceeds from an account of the beginnings of chemistry, which is cryptically and incorrectly said (p. 177) to have been founded by Jabir, to the work of Dalton. The study of the oxides of nitrogen, 'muriatic' acid and the halogens, sulphur, etc., then follows, and by p. 179 the structure of the atom is attained. The rest of the book deals with practical work, and several interesting experiments are described. There are many excellent illustrations, including portraits of Boyle, Priestley, and Lavoisier.

Even those teachers who do not believe in the rather literary method of introduction to the science here adopted will find Mr. Vernon's book interesting and suggestive. The historical details appear to have been compiled from trustworthy sources, although the judgment passed (on p. 19) on the Alexandrian School is somewhat startling.

A Dictionary of Applied Chemistry. By Sir Edward Thorpe; assisted by eminent contributors. Vol. 6: S. Acid to Tetryl. Revised and enlarged edition. Pp. viii+791. (London: Longmans, Green and Co., Ltd., 1926.) 60*s.* net.

THE characteristic features of the new volume of Thorpe's "Dictionary" are so similar to those of the preceding volumes that very little comment is possible. There has been a considerable expansion, from 572 to 791 pages, in the new edition, but this has been distributed with remarkable uniformity, and the reviewer has been unable to discover any striking novelties in the present volume, except that the 'article' on tetryl, which in the old edition was confined to the single word '= butyl,' now contains an account of an important auxiliary explosive. Articles on sodium, sulphur, sulphuric acid, synthetic drugs, and tartaric acid have been expanded considerably, but again mainly by interlining with new matter rather than by additions in bulk. It, therefore, only remains to express appreciation of the completion of what is presumably the penultimate volume of the "Dictionary."

Photochemical Reactions in Liquids and Gases: a General Discussion held by the Faraday Society, October 1925. Pp. 435-658. (London: Gurney and Jackson, 1926.) 15*s.* 6*d.* net.

WITH the natural growth of the experimental sciences, the gaps between physics and chemistry somewhat unexpectedly expand rather than contract. From time to time, however, from one side or the other, some new point of view is presented, and in a few years a large portion of this 'terra incognita' is explored and mapped out. In support of this thesis

may be mentioned the recent rapid advance in knowledge of the behaviour and properties of interfaces, a subject which until only some ten years ago appeared to be but a sterile flower cultured by pure physics.

The action of radiation in promoting chemical action has long been an interesting experimental field for chemists, but it cannot be said that much real knowledge has been reaped as a result of their labours. During the last few years, however, physical investigation has revealed certain apparently fundamental laws in the interaction between radiation and matter, and has given us a definite concept of the 'active' molecule postulated by Arrhenius. It is in the application of these laws to photochemical action that we may anticipate a revival of interest in photochemistry.

The Faraday Society has since its inception pursued the policy of promoting discussions, international in character, so as to obtain not only a record of the growth of these various physicochemical subjects, but also to apply the necessary stimulus of interest. The contents of this volume certainly fulfil these conditions. In it we find the signs of the revival of photochemistry, for nearly all the contributions deal with the mechanism rather than with the products of reactions. Whilst it is clear that certain reactions which are simple from the chemical point of view are in reality extremely complex, yet hope is offered from the fact that even complex chemical reactions may result from a series of physical actions which are capable of isolation and identification. It is also evident that a very close collaboration of physicist and chemist will be necessary to make real progress in photochemistry, and to prepare the way for the plant biologist. The Society is warmly to be congratulated for the success it has achieved.

E. K. R.

Practical Physiological Chemistry. By Sydney W. Cole. Seventh edition. Pp. xii+481. (Cambridge: W. Heffer and Sons, Ltd.; London: Simpkin, Marshall and Co., Ltd., 1926.) 16*s.* net.

THE fact that this well-known work has reached its seventh edition is its own recommendation. Its usefulness has been enhanced by the inclusion of accounts of electrical methods for the measurement of hydrogen ion concentration, and of chapters on biological oxidations and reductions and on the analysis of the blood. The work itself is more suitable for the advanced student and the research worker than for the ordinary medical student, though by judicious selection of material a course for the latter could be easily arranged. The directions for all practical exercises are given in very full detail, preceded in each case by a short theoretical account of the subject. Among the more recent additions, the following appear to be especially noteworthy: the description of the hydrogen and quinhydrone electrodes, the account of glutathione in the chapter on oxidations, and the description of the Hagedorn-Jensen method of estimating the sugar of the blood. The inclusion of a method may be considered as a guarantee of its trustworthiness, provided that the details are faithfully followed.

Practical and Applied Mathematics.

Advanced Calculus. By Prof. William F. Osgood. Pp. xvi+530. (New York: The Macmillan Co., 1925.) 25*s.* net.

PROF. OSGOOD has here given us a very valuable book on the calculus. It will be useful "not merely to the specialist in mathematics or physics," but "to all who would possess themselves of the calculus as a method for understanding . . . the quantitative relations which follow from the laws of Nature." There are excellent chapters, to mention only a few, on the general methods of integration, with multiple integrals and their transformation; elliptic integrals; vector analysis; the calculus of variations; and finally, a sketch of the theory of functions of a complex variable. The whole is treated with modern rigour and with special reference to physical applications, of which there are many. There are numerous examples for practice together with answers, and the book is excellently produced.

Advanced Calculus: a Course arranged with Special Reference to the Needs of Students of Applied Mathematics. By Prof. Frederick S. Woods. Pp. ix+397. (Boston, New York and London: Ginn and Co., 1926.) 21*s.* net.

THIS book has been written for those students who are "chiefly interested in the applications of the calculus," but are not "primarily concerned with theoretical questions." The author has, however, wisely included some essential theory not only to render the practical rules intelligible, but also "to introduce the students to theoretical questions and possibly to incite in some a desire for more thorough study." The work ranges from a discussion of elementary functions to partial differential equations, Bessel functions, the calculus of variations, and elliptic integrals. There is a good chapter on the functions of a complex variable, and the whole text throughout is well illustrated by practical examples.

Practical Mathematics. By A. Dakin. (Bell's Mathematical Series for Schools and Colleges.) Part 2. Pp. viii+363-629+xxv-xxxiv. (London: G. Bell and Sons, Ltd., 1926.) 4*s.* 6*d.*

MR. DAKIN'S useful little book completes a course specially planned to give practical students a sound knowledge of fundamentals. The treatment is designed to be of real educational value, and the judicious combination of experiment, deduction, and concrete application is a highly commendable feature.

Mechanics and Applied Mathematics: Statics—Dynamics—Hydrostatics. By W. D. Hills. Part 2: *Applied Mathematics.* Pp. xi+248. (London: University of London Press, Ltd., 1926.) 5*s.*

THIS volume completes the course, begun in Part 1, to cover the London Intermediate Science syllabus. The author has well carried out his aim "to temper theory and practice." The numerous worked examples are clearly explained and, with the text, excellently illustrated. The book should prove very useful.

Geometry.

(1) *Practical Geometry: based on the various Geometry Books by Godfrey and Siddons.* By A. W. Siddons and R. T. Hughes. Pp. x+264. (Cambridge: At the University Press, 1926.) 4*s*.

(2) *Theoretical Geometry: based on the various Geometry Books by Godfrey and Siddons.* By A. W. Siddons and R. T. Hughes. Pp. xvi+173. (Cambridge: At the University Press, 1926.) 3*s*.

THESE two books are intended to be used together in order to give a sound course in the methods of geometry.

In the practical geometry, intuition and experiment are skilfully employed to develop the power of intelligent inference.

In the volume on theoretical geometry, the Assistant Masters' Association's sequence of theorems has been followed, the object being to cover the requirements of the non-specialist. The whole treatment is so good that many teachers will regret the omission of the powerful method of limits in its application to tangency.

Analytic Geometry. By Prof. Maria M. Roberts and Prof. Julia T. Colpitts. Second edition. Pp. xii+261. (New York: John Wiley and Sons, Inc.; London: Chapman and Hall, Ltd., 1926.) 9*s*. net.

THE courses given to students of engineering and science at the Iowa State College form the basis of this book, which embraces Cartesian geometry in two and three dimensions. Special emphasis is placed upon those portions of the subject essential to a working knowledge of the calculus. The treatment is lucid and practical, and there are numerous exercises designed to stimulate independent thought.

Miscellany.

Teaching Science in Schools. By John Brown. Pp. x+170. (London: University of London Press, Ltd., 1925.) 3*s*. 6*d*. net.

THE author of this small book is, as a critical onlooker in the rôle of a London County Council inspector of schools, eminently qualified in taking upon himself the task of a review of the present position of the teaching of science in schools. Much that he has to say is primarily the concern of the elementary-school teacher. Nevertheless, there is much that is common in aims and ideals and methods as between the elementary and the secondary school, and the teaching profession owes its thanks to Mr. Brown for a most valuable contribution to the literature of educational method.

A careful reading of his work impresses one vividly with a sane sense of balance. The swing-swang of the pendulum of method as between too much and too little formal laboratory work rightly has very little sympathy from the author. The pendulum must be damped down to its mean position. At the same time, we are bound to submit that there is no sign of an indifference to a rational realisation of the need for improvement on the part of teachers of science as a whole such as might be inferred to exist from a reading of this book. On the contrary, so far as secondary schools are concerned, there are few 'subject' organisations that compare either in vigour, effect, or for that matter in numerical strength, with, for example, the Science Masters' Association. Most of the points and submissions contained in Mr. Brown's book have been or are being thrashed out by the science teachers of today. Nevertheless, this in no way detracts from the value of a book that succeeds admirably in placing before the reader a résumé of the problems that confront teachers of science, and the lines along which a sound solution may be found. I. B. H.

The Journal of the Institute of Metals. Edited by G. Shaw Scott. Vol. 35, No. 1. Pp. xii+988. (London: The Institute of Metals, 1926.) 31*s*. 6*d*. net.

A SUBJECT which is assuming great importance in metallurgical discussions is that of 'creep' at high temperatures, and the new volume contains two papers on this topic. R. W. Bailey reviews the experimental evidence and supplies the first real attempt to provide an explanation, whilst H. J. Tapsell and J. Bradley contribute the results of experiments with an alloy of nickel and copper. The constitution of the alloys of silver and tin, which is of importance for the knowledge of dental alloys, has been determined satisfactorily by A. J. Murphy, previous determinations having been badly in error, and another careful alloy investigation is that of the aluminium-copper-tin alloys rich in copper, by D. Stockdale. W. Hume-Rothery makes a bold and ingenious attempt to apply the Bohr theory of the atom to intermetallic compounds, supporting his theoretical work by interesting experimental data. The speculations have given rise to much discussion, but chemists will follow with interest an effort to find some system in these peculiarly puzzling compounds. Among the more practical papers, a useful account of the die-casting of aluminium alloys, by G. Mortimer, may be mentioned. The May lecture this year was by Prof. Carpenter, who dealt with some of the properties of single crystals. The high standard reached by several of the contributors in their photomicrographs is to be noted with satisfaction. As usual, the volume contains a very extensive and exceptionally valuable collection of abstracts.

A Catalogue of British Scientific and Technical Books. Supplement, 1925. Arranged by Daphne Shaw. Pp. viii+166. (London: British Science Guild, 1926.) 2*s*. 6*d*. net.

BOOK catalogues intended to occupy a permanent place on the reference shelves of libraries must be kept closely up-to-date, if the hopes of their compilers are to be realised. We are, therefore, pleased to note that the British Science Guild has found the means to publish a supplement containing 2258 entries completing its useful Catalogue of 1925, and that it further proposes to print annual supplements early in each year if sufficient support is obtained for these issues. Though the change of system of publication from the class to the dictionary order of headings has advantages, the break in the continuity of system in a serial publication may be resented by its users. The present dictionary headings and references are also not up to the standard of modern library practice. The entries, however, which have been taken from the monthly lists in NATURE, are admirably full, and the allotment of entries to their respective headings is generally satisfactory.

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Supplement to NATURE

No. 2979 DECEMBER 4, 1926

Recent Developments of Cosmical Physics.[1]

By J. H. JEANS, Sec. R.S.

UNTIL recent years astronomy was concerned almost entirely with the sun, moon, and planets; the stars were mere points of light so inconceivably remote as to be of only minor interest. To-day Urania has wearied of the speck of dust we call the solar system, and claims the whole universe for her playground; the astronomer's interest centres almost exclusively on the stars. The dynamical astronomer, for example, having lost interest in the motions of the planets and their satellites, studies the arrangement and motions of the stars in the hope of discovering the general plan at least of the architecture and mechanism of the universe; for him the whole universe is a single dynamical system formed of innumerable particles—the stars—each of which attracts each other according to the universal law of gravitation.

The physical astronomer finds a different interest in the stars. For him each separate star is a complete physical system: it is a crucible in which matter is subjected to temperatures and pressures far beyond any available to the terrestrial physicist. From a study of the radiation emitted by the stars, the physical astronomer tries to unravel their physical structure, to discover how they generate their energy, and by what mechanism this energy is transmitted to their surfaces and discharged into space as radiation. In this way he may perchance happen upon properties of matter which have eluded the terrestrial physicist owing to the small range of physical conditions at his command. If the simile may be pardoned, on the plea that it is at least true to scale, the animalculæ which inhabit a raindrop may learn something of the properties of water by manipulating the particles of the raindrop with their puny strength, but they may also learn something by watching the uncontrollable fall of torrents over Niagara. The primary study of the physical astronomer is precisely this external fall of torrents over Niagara; his ultimate aim is to weld cosmical physics on to terrestrial physics so as to form one all-embracing science. Only when this has been done will it be possible to understand the main trend of events in the physical universe.

[1] A lecture delivered at University College, London, on November 9, 1926.

The Interpretation of Stellar Spectra.

There is only one method available to this end, namely, a study of the radiation emitted by different stars. Apart from unscientific speculations, physical astronomy may be said to have come to birth in 1863, when Huggins attached a spectroscope to a telescope and found that certain lines in the spectra of the stars were identical with lines emitted in the laboratory by the known chemical elements. The early stellar spectroscopists believed that they were investigating 'the chemistry of the stars' although we know now that they were merely opening up a fundamental problem of the physics of the stars. The spectrum of Sirius, for example, was found to exhibit hydrogen lines very strongly and calcium lines very weakly; in the solar spectrum the relative strength of these two sets of lines was reversed, calcium being strong and hydrogen weak. They concluded that hydrogen was specially prominent in the constitution of Sirius and calcium in that of the sun. Believing that Sirius must one day develop into a star similar to our sun, they conjectured that its substance must gradually change from hydrogen into calcium and other more complex elements, thus finding support for the long-established hypothesis that the more complex elements were formed by gradual evolution out of the simplest.

The true interpretation of these early observations, as the investigations of Saha, R. H. Fowler and Milne have abundantly proved, is merely that the surface of Sirius is at a temperature at which hydrogen is specially active in emitting and absorbing radiation, while the sun's surface is at a lower temperature at which hydrogen is comparatively inert, calcium, iron, etc., having become active in its place. Just as the laboratory physicist can produce different spectra from the same vacuum-tube by varying the mode and conditions of excitation, so Nature produces different spectra from the same stellar material by varying its temperature.

Clearly this circumstance robs stellar spectra of all direct evolutionary significance. The spectra of the stars merely tell us their present surface temperatures, so that even if we could arrange the stars in order of age, a comparison of their spectra would only show

whether their surfaces were becoming hotter or cooler; it would give no information as to chemical changes occurring in their substance.

The Sizes of the Stars.

The knowledge of a star's surface temperature nevertheless opens the door to further valuable knowledge. The hotter a surface is, the more energetically it radiates heat, and from a knowledge of a star's surface temperature it is easy to calculate its radiation per square inch of surface—the sun, for example, radiates approximately 560,000 calories a minute, which is about the energy output of a 50 horse-power engine, from each square inch of its surface. The hottest stars of all probably radiate at least a thousand times as much per square inch of surface.

In this way we can estimate a star's radiation per square inch of its surface. We can also estimate the radiation from its whole surface; this can be calculated at once from its distance and apparent brightness. Simple division gives the area of the star's surface, and hence its radius and volume. Calculated stellar radii range from about 300 times the sun's radius for Betelgeux to about 0·03 times the sun's radius for the companion of Sirius.

As is well known, the diameters of certain stars have recently been observed directly with the Michelson interferometer, and the measured values agree almost perfectly with those calculated in the simple way just explained. The interferometer method is only available for the largest stars, but at the extreme other end of the scale the theory of relativity has come to the rescue. The shift of spectral lines toward the red, which Einstein predicted to be a necessary consequence of the theory of relativity, has been observed in the light received from the companion of Sirius, and its amount corresponds exactly to the radius calculated for the star in the way just explained. So much of a sensational kind has been written about the observational measurements of the diameters of Betelgeux and of the companion to Sirius, that it may be well to remember that, while the *methods* were novel and of the greatest interest and importance, the *results* were precisely those that were generally expected, and such as a simple arithmetical calculation showed to be practically inevitable. Indeed, this calculation could only have failed in one way. It is based on the assumption that the surfaces of the stars emit their full temperature-radiation like the surface of the sun. If the stars had been transparent bodies like the planetary nebulæ, or solid bodies like the moon, this assumption would have been false, and the observations would have revealed its falsity.

Our gain of positive knowledge from these observations is that Betelgeux and the companion to Sirius are neither transparent nor solid bodies, but full radiators like the sun. Moreover, as the three stars just mentioned are about as different as any three stars possibly could be, representing approximately the two extreme ends and the middle of the scale in almost any ordered arrangement we please to make, it seems reasonable to suppose that all stars are full radiators and so, as regards their mechanism of radiation, are essentially similar structures.

The Physical State of Stellar Interiors.

What, then, is this mechanism of radiation? And what, as a preliminary question, is the physical state of stellar matter? In the early days of spectroscopy it was commonly supposed, from a faulty analogy with laboratory experiments, that a hot gas always gave a line spectrum, and that a continuous spectrum, such as is exhibited by the stars, could be emitted only by a solid or a liquid body. It is now generally conceded that this view was erroneous, and it is recognised that the continuous spectrum of a star merely indicates that the star is not transparent, thus leaving the question of stellar structure almost entirely open.

The view of stellar structure now universally accepted is that stars are formed of matter which, as a consequence of its high temperature, is to a very large extent broken up into its constituent electrons and nuclei, these all moving about independently like the molecules of a gas. The electrostatic attractions which in more peaceful surroundings would rapidly unite the wandering nuclei and electrons into complete atoms and molecules, are powerless in the general whirl of rapidly moving projectiles and in face of the shattering blows of the quanta of high-frequency radiation which the high temperatures of the stellar interiors generate. When I first put forward this view in 1917 (Bakerian Lecture, *Phil. Trans.*, **218**, p. 209), I thought it was entirely novel, but I have since found that in 1644 Descartes had conjectured that the sun and fixed stars were made of matter "which possesses such violence of agitation that, impinging upon other bodies, it gets divided into indefinitely minute particles." My own suggestion was not conjecture, being based on incontrovertible scientific grounds. In 1907 Emden had published calculations ("Gas Kugeln," p. 96) on the interior states of the sun and stars, in which he assumed the stars to be masses of gas resting in an equilibrium similar to that of the lower strata of the earth's atmosphere—the so-called 'adiabatic' equilibrium in which there are assumed always to be sufficient currents to keep the constituent gases thoroughly mixed by a process of stirring. On this supposition he found that

if the sun were composed of air or other diatomic gas of equal molecular weight, its central temperature would be 455 million degrees, while if it consisted of hydrogen, or other diatomic gas of molecular weight 2, its central temperature would be 31·5 million degrees. These temperatures are so high that no atom or molecule could survive them; at 31·5 million degrees the quantum of radiation has energy $2{\cdot}1 \times 10^{-8}$ ergs, which is sufficient to move an electron through a potential difference of 13,500 volts. Even with such quanta flying about, the atomic nuclei are still safe, far higher than stellar temperatures being needed to dissociate these into their constituent electrical charges, but the electrons must of necessity nearly all be torn off atoms of moderate atomic weight and the nuclei left almost or entirely bare.

To a first rough approximation we may regard stellar matter, at any rate in the star's hot central regions, as consisting of a mixture of bare nuclei and free electrons. Passing outwards towards the star's surface, the temperature falls, and we come to atoms which are more and more fully formed, until finally, close to the surface, we meet atoms which are completely formed except perhaps for one or two of their outermost electrons. In the surfaces of the coolest stars of all, we even find complete molecules, as, for example, the molecules of titanium oxide and magnesium hydride, which appear in the spectra of certain classes of stars.

The Mechanism of Stellar Interiors.

The mixture of free electrons and bare nuclei or imperfectly formed atoms will behave like a mixture of monatomic gases. In completely broken-up hydrogen, each hydrogen molecule gives rise to four flying units—two protons and two free electrons—so that the effective molecular weight of the mixture will be 0·5. The corresponding figure for helium is 1·33, for calcium 1·90, for iron 2·07, and for lead 2·50, but since atoms of lead would not be completely broken up at stellar temperatures, the actual value for stellar lead would be somewhat higher. If we momentarily adopt 2 as a mean molecular weight of stellar matter, we find that Emden's calculations give 31·5 million degrees for the central temperature of the sun if formed of hydrogen molecules (mol. wt. 2). Various adjustments must be made in this figure, but they are of comparatively minor importance, and Emden's original figure of 31·5 million degrees is probably not very far from the actual temperature of the sun's centre. Indeed, Russell has recently suggested that the great majority of stars have central temperatures fairly close to 32 million degrees (Nature, Aug. 8, 1925).

One of the necessary adjustments arises from Emden's calculations having neglected the pressure of radiation in stellar interiors.[2] At 31·5 million degrees the pressure of radiation is about 2500 million atmospheres. Huge though this is in comparison with terrestrial pressures, it is only some five per cent. of the ordinary gas-pressure of the broken-up atoms and electrons at the sun's centre. We could allow for its dynamical effects by decreasing our assumed mean molecular weight by 5 per cent., but this mean molecular weight is not in any case known to within 5 per cent. In exceptionally massive stars, the pressure of radiation assumes somewhat greater importance. For example, at the centre of a star of some ten times the sun's mass, radiation pressure is about equal to gas-pressure. To allow for its effects in this case we should have to suppose the assumed mean molecular weight halved—reduced perhaps from 2 to 1. In every case we shall get a true picture of stellar structure if we think of the layers of stellar matter as held up against gravitation by the incessant impact of a certain number of atomic nuclei or partially stripped atoms, the 'molecular weight' of which is practically the same as that of the corresponding complete atoms, together with a far greater number of free electrons of standard 'molecular weight' 0·00055, and a rather small number of 'molecules' of radiation the molecular weight of which is negligibly small. The combined impacts of these three types of projectiles prevent the star from falling in under its own gravitational attraction.

This gives us, I think, the best snapshot picture of a star's structure. The corresponding picture of its mechanism is obtained by thinking of the nuclei as α-ray particles, of the free electrons as β-ray particles, and of the radiation as γ-rays (although in most stars the main bulk of the radiation has the wave-length of X-rays); and, precisely as in laboratory work, the β-rays are more penetrating than the α-rays, and the γ-rays are more penetrating than either.

The Transport of Energy inside a Star.

In ordinary kinetic theory of gases, conduction of heat is studied by regarding the molecules of the gas as carriers of energy; each molecule has a carrying power which is jointly proportional to its heat energy, its velocity and its free-path. In the interior of a star there are, as we have seen, three distinct types of carriers—the nuclei (or atoms), the free electrons, and the radiation. We can compare the relative carrying capacities of these three types of carriers by multiplying up the energy, velocity, and free-path of each.

[2] I first directed attention to this in reviewing Emden's book (*Astrophys. Jour.*, 30 (1909), p. 72, and gave a reasonably accurate estimate of the ratio of this pressure to ordinary gas-pressure in stellar interiors in 1917 (Bakerian Lecture, May 17, 1917, p. 209). Some months previously Eddington had given an estimate which made this ratio some hundreds of times too large. He corrected this at the earliest opportunity (*Mon. Not. R.A.S.*, June 1917), but not in time to overtake sensational statements, still occasionally encountered, that pressure of radiation is of predominant importance in the dynamics of stellar interiors.

The nuclei and the free electrons have, of course, quite definite free-paths. The same is true of the radiation if this is regarded as consisting of discrete quanta; when a quantum is emitted a free-path begins, and when it is re-absorbed the free-path ends. Whether we think in terms of undulatory theory or quanta, we may suppose that a beam of radiation is reduced in intensity by a factor $e^{-k\rho x}$ on passing through a thickness x of matter of density ρ, where k is the 'coefficient of opacity' of the matter. By comparison with the kinetic theory formula $e^{-x/\lambda}$ for the reduction in strength of a shower of moving molecules, we see that the 'free-path' of the radiation must be taken to be $1/k\rho$. When we use this value for the free-path of radiation and calculate carrying capacities in the way already explained, the carrying capacity of both nuclei and electrons is found to be insignificant in comparison with that of the radiation. The nuclei and electrons may have the greater amount of energy to carry, but the distance over which they carry it, their free-path, is far less than that of the radiation, and their speed of transport is also less, since radiation transports energy with the velocity of light. In this way it comes about that practically the whole transport of energy from the interior of a star to its surface is by the vehicle of radiation.

This general principle was first clearly stated by Sampson in 1894 (*Mem. R.A.S.*, **51**, p. 123), but his detailed applications were vitiated by his using an erroneous law of radiation. Twelve years later, Schwarzschild independently advanced the same idea (*Gött. Nach.*, 1906, p. 41); he showed how the temperature of any element of a star's interior must be determined by the condition that it received just as much radiation as it emitted, and gave accurate equations of radiative equilibrium which have formed the basis of every subsequent discussion of the problem.

The Configurations of a Star in Equilibrium.

As a consequence of radiation completely outstripping the material carriers in the transport of energy to the star's surface, the build of a star is entirely determined by the values of k, the coefficient of opacity in its interior. If this coefficient is everywhere zero, the star is entirely transparent, and so cannot retain any heat; we now have a star of zero temperature and therefore of infinite extent. If, on the other hand, the coefficient of opacity is everywhere infinite, the star is completely opaque, so that all radiation accumulates where it is generated until the star's temperature becomes infinite, and we have a star of infinite temperature but of infinitesimal radius. It is, of course, the intermediate values which are of practical interest, but the two extreme cases just mentioned show how the whole build of a star depends on the value of the opacity coefficient k. So much is this the case that all attempts to investigate the build of stars before the value of this coefficient was known can only be regarded as speculation.

The first attempt to evaluate it theoretically by Eddington in 1922 (*Mon. Not. R.A.S.*, **83**, p. 32) proved unsuccessful and was withdrawn. In the next year Kramers (*Phil. Mag.*, **46**, p. 836) put forward the theory of opacity which has now gained general acceptance. Using the value of the opacity coefficient given by this theory, it is possible to determine the complete build of a star having any given mass and any given rate of generation of energy. In this way I have shown (*Mon. Not. R.A.S.*, **85**, pp. 196 and 394) that a star of given mass can rest in equilibrium with any radius from zero to infinity, different radii corresponding to different rates of generation of energy from zero to infinity by the star. A star adjusts its radius to suit its rate of generation of energy, and in so doing fixes its surface temperature and spectral type. If a star's rate of generation of energy were suddenly to change, the star would expand or contract until it had assumed the radius and temperature suited to its new rate of generation of energy. Contrary to common belief, an increase in a star's rate of generation of energy causes it to contract its radius and increase its temperature, while a slackening in its generation of energy is found to result in expansion and cooling. Thus we see the giant red stars such as Betelgeux do not owe their immense size to their radiating so much energy, but to their radiating so little; indeed, comparatively compact stars such as Plaskett's star and V Puppis are radiating far more in proportion to their masses. The general theoretical principle can be verified by the examination of pairs of stars of approximately equal mass, as for example the two pairs in the following table. The surface temperatures are here deduced directly from the observed spectra, the radii then being calculated in the way already explained.

Star.	Mass (in terms of sun).	Generation of energy per gram (ergs per second).	Observed temperature.	Radius (in terms of sun).
Sun	1·00	1·9	5750	1·00
α Cent. B	0·97	1·4	3700	2·03
Procyon	1·13	10·2	8300	1·17
α Cent. A	1·14	2·3	5000	1·56

Stellar Evolution.

There is not likely to be any abrupt change in the rate of generation of energy of an actual star. There will be a slow secular decrease, but this will be associated with a slow secular decrease of the star's mass

resulting from its continual emission of radiation. For example, the 560,000 calories of radiation which stream out every minute from each square inch of the sun's surface have a mass of $2{\cdot}5 \times 10^{-8}$ gm., whence it is readily calculated that the sun's mass must diminish by 250 million tons every minute. After millions of millions of years this rate of wastage produces an effect even on the gigantic mass of the sun. To trace the changes in the radius and temperature of an actual star we must study the sequence of configurations assumed in turn as the mass and the rate of generation of energy change together. In this way I have found (*Mon. Not. R.A.S.*, January 1925) that a normal star would first decrease in size and get hotter, but would ultimately expand and get cooler again. This result provides a simple dynamical interpretation of the sequence of 'ascending and descending temperatures' which was first suggested by Lockyer, and formed the outstanding feature of Russell's 1913 theory of stellar evolution—although our physical interpretation is very different from that suggested by Russell.

The Atomic Weight of Stellar Matter.

In the simplest case, in which energy is generated uniformly throughout a star's mass, the surface temperature T assumed by a star of mass M and of given luminosity (or rate of generation of energy) is given by the equation—

$$\text{Star's luminosity} = C\left(\frac{N^2}{A}\right)^{-0{\cdot}8} T^{0{\cdot}8}\mu^{6{\cdot}8} f(M).$$

Here C is a known constant, N and A are the atomic number and atomic weight of the stellar atoms, and μ the effective molecular weight (about 2) of the broken-up stellar material; T is the temperature of the star's surface and $f(M)$ is a quantity I have calculated and tabulated, which depends only on the star's mass (*Mon. Not. R.A.S.*, **85**, p. 395).

The quantity N^2/A necessarily occurs in the foregoing formula, because the coefficient of opacity, by which the star's whole structure is determined, is proportional to N^2/A. If a Maxwell demon could cut every atomic nucleus in a piece of matter into two equal halves, he would halve both N and A and so also N^2/A, with the result that the substance would become twice as transparent as before. This shows that a large clot of matter in the form of a massive nucleus is far more effective in absorbing X-radiation than a large number of small clots of equal total mass. It is for this reason that the physicist and surgeon both select lead as the material with which to screen their X-ray apparatus; a ton of lead is far more effective in stopping unwanted X-rays than a ton of wood or of iron. If we knew the strength of an X-ray apparatus, and the total weight of shielding material round it, we could form a very fair estimate of the atomic weight of the shielding material by measuring the amount of X-radiation which escaped through it.

A very similar method may be used to determine the atomic weight of the atoms of which the stars are composed. A star is in effect nothing but a huge X-ray apparatus. We know the total mass of many stars, and we can readily calculate the rate at which they are generating X-rays—it is merely the rate at which they are radiating energy away into space. If we could shut our Maxwell demon inside a star and make him cut each atomic nucleus in half, keeping the star's mass and rate of generation unaltered, we should halve the coefficient of opacity of the star. This would necessitate a change in the star's build: in actual fact its radius would increase fourfold while its surface temperature would be halved. We could follow the progress of the demon's work by watching the changes in the surface temperature of the star. Hence from the observed surface temperature of any star the mass and luminosity of which are known, it must be possible to estimate the atomic weight of the atoms of which the star is composed. The formula given above provides the means.

I ought perhaps to mention in passing that Eddington and others have approached this question from the other end, assigning conjectural values to N^2/A from our knowledge of the elements which occur in the atmospheres of the sun and stars. Such a course appears to be very risky. A star's spectrum gives no indication of the selection of elements which occur in its interior; and there is at least an *a priori* possibility that the elements occurring there may be entirely different from those which appear in its surface; consider into what errors an extra-terrestrial observer might be led if he assumed that the earth contained no chemical elements beyond those appearing in its atmosphere.

When, however, the risk has been taken, and such values assigned to N^2/A, all the quantities which occur in the luminosity formula are known, and the only question which remains is whether the values calculated for the luminosity agree with those observed through the telescope. They do not.

It is clear that the value of N^2/A must be adjusted until agreement is obtained, and this amounts to precisely the same thing as determining N^2/A, directly and at once, from the luminosity formula. On doing this for a series of stars, I have found (*Mon. Not. R.A.S.*, June 1926) that two very significant facts emerge. First, most of the values so determined prove to be higher than the value for uranium, the heaviest element known on earth. Second, the different values of N^2/A show an ordered arrangement, the youngest stars

generally giving the highest values for N^2/A, and this value falling as we pass to older stars.

The second of these results has far-reaching implications. Contrary to the views of the early spectroscopists, and contrary to what is still probably the prevalent belief, it now looks as though the atoms in a star become simpler as the star grows older; evolution appears to be from complex to simple, and not, as in biology, from simple to complex. There is at present no direct experimental evidence bearing on this question except that provided by radioactivity, where evolution is certainly from complex to simple, atoms of lower atomic weight being continually produced by the disappearance of atoms of higher atomic weight.

The evidence of physical astronomy, pointing to an evolution of matter in the same direction, suggests that the main evolution of matter in the universe may be of the same type as, but a generalisation of, the radioactive processes as they occur on earth. The evidence so far given has been based entirely on Kramers' theory of opacity for X-radiation. This theory has been found to agree very well with the observed absorption in the laboratory of radiation of about the wavelength which occurs in stellar interiors, while its theoretical basis has been discussed fully and critically by Eddington, Milne, and others, who have been unable to suggest any substantial alteration. Still, if the evidence from Kramers' formula were the only evidence available, our conclusions would be open to the charge of resting, if not on a slight, at least on a single, foundation. But there is plenty of further evidence, as we shall now see.

Distribution of Chemical Elements in a Star.

A star necessarily arranges itself so that there is a great concentration of matter near its centre. This is primarily a consequence of the inverse square law of gravitation, although the opacity law is involved also to some extent. With Kramers' formula for the opacity the arrangement is such that the central density is 100 or more times the mean density, while at least some 90 or 95 per cent. of the star's total mass is concentrated in a sphere of half the radius, and so of only one-eighth the volume, of the star. But the degree of central condensation is rather insensitive to changes in the opacity formula, and any reasonable formula would still give very high central condensation. A strict mathematical argument based on this circumstance (*Mon. Not. R.A.S.*, June 1926, p. 561) enables us to rule out the possibility of convection currents stirring up a star's interior in the way in which boiling water is stirred up in a kettle. Convection occurs in a kettle because the hot water at the bottom is of lower density than the cool water at the top; it is absent in a star because the hot matter near the centre, notwithstanding its intense heat, is still far, far denser than the cool matter near the surface. Thus, the mixture of matter in a star's interior is not analogous to that in the earth's lower atmosphere, where the constituent gases are kept thoroughly mixed by winds and storms, but rather to a serene upper atmosphere in which the lightest elements float to the top while the heaviest sink downwards under gravity.

Such considerations as these suggest at once that the elements which indicate their presence in the spectra of the outermost layers of the sun and stars are only the very lightest of the series of elements existing in the star. It is natural that the earth, formed originally out of the sun's outer layers, should contain precisely the same chemical elements as these outer layers, but it now appears that there ought to be heavier elements inside. The calculation which assigns to stellar matter atomic numbers higher than that of uranium no longer looks suspicious or paradoxical; it begins to look natural, and indeed almost inevitable.

The Generation of Energy in a Star.

Further evidence that the atomic weights of stellar atoms are higher than those of any known terrestrial atoms may be obtained by considering the rate of generation of energy inside a star. The sun radiates energy at about 2 ergs per second for each gram of its mass, and so must generate energy at this rate in its interior. To the best of our knowledge it has generated and radiated at this, or a greater, rate for some millions of millions of years. Could the sun have any such radiating capacity if its interior were formed of the common terrestrial elements, calcium, iron, silicon, etc.?

One's first impulse is to say, No. Even if the sun were built of pure uranium, its radiating power would be only about one-half of that observed, and would only last for a minute fraction of what is believed to have been the sun's life. A sun of pure radium would radiate more than enough for the moment, but its life would be limited to a few thousand years. No possible combination of terrestrial elements can give the combination of high radiation and of staying power which is observed in the sun and stars.

We must, however, remember that stellar interiors are at pressures and temperatures which are quite unattainable in our laboratories. We are led to wonder whether our terrestrial elements would behave quite differently if they were exposed to stellar conditions. Is it possible, for example, that the sun's interior is formed of ordinary terrestrial elements, which owe their high generation of energy merely to their high temperatures and pressures?

A general survey of astronomy throws a good deal of

light on this question. We find immediately that the stars which radiate most energetically (per unit mass) are not, broadly speaking, the hottest stars, and neither are they the densest. Some of the hottest and densest stars are entirely put to shame in the matter of radiation by very cool stars of low density such as Antares and Betelgeux. If we arrange the stars in order of radiation per unit mass, we shall find that we have arranged them neither in order of temperature nor of density, but very approximately in order of age; the youngest stars radiate most energetically, regardless of their interior temperatures and density; the older stars appear to be tired out.

The general tendency is shown in the following table:

Star.	Generation of energy (ergs per gram).	Central temperature.	Central density.	Age.
Plaskett's Star	1000	500,000,000	Very great	Less than 10^{11} yr.
V Puppis	640	300,000,000	More than 1,000	
Antares	320	1,000,000	0·005	
Capella A	50	8,000,000	0·5	Less than 10^{12} yr.
Sirius	21	150,000,000	1,000	10^{12} yr.
Sun	1·88	70,000,000	300	7×10^{12} yr.
α Cent. B	1·39	15,000,000	10	7×10^{12} yr.
Kruger 60 B	0·02	70,000,000	30,000	Very old.
Sirius B	0·003	Unknown	More than 53,000	Unknown.

If it is asked whether densities so high as these can really exist at the centres of the stars, the answer is provided by the companion to Sirius (Sirius B). Direct observation has shown that the *mean* density of this star is about 53,000, and the central density must of course be higher. Incidentally, as Eddington has remarked, this provides striking confirmation of our view that stellar matter consists of atoms broken up into their fundamental constituents. It is impossible to compress matter formed of complete atoms of radii 10^{-8} cm. or more to anything approaching these high densities, but there is no difficulty as regards minute nuclei and electrons of radii of the order of 10^{-13} cm.

It has to be admitted that many of the entries in the table are highly conjectural, and few can claim any great accuracy. But while many astronomers may prefer different values for individual entries in the table, I doubt if any would seriously challenge the general contention that a star's energy-generating capacity depends primarily on its age, and not, at any rate primarily, on its central temperature or density.

No doubt exceptions to the general rule can be found. An extreme example is provided by the earth and sun; the matter of which these two bodies are formed must be of the same ultimate age, yet they radiate at very different rates per unit mass. This is readily explained if we suppose the heavy atoms from which the sun's energy originates to have sunk deep into its interior, and so not to have entered into the composition of the earth and planets: a similar explanation will account for the different radiating capacities of the components of binary systems. But these exceptions result from special conditions prevailing in special cases; they do not affect the general law that a star's generation of energy is not determined by either its density or its temperature.

The accompanying table shows that the law is well supported by observational astronomy; it can also be reached from a theoretical study of the actual process of generation of energy in a star. A mass of evidence, mostly dynamical, indicates that the stars must have existed for millions of millions of years. To take one example, newly formed binary stars have circular, or nearly circular, orbits; this is a consequence of the manner of their formation. Every gravitational pull on a circular orbit tends to make the orbit more elliptical, so that the older a binary star is, the more elliptical its orbit ought to be. This is actually found to be the case. But from our general knowledge of the number and masses of the stars wandering about in space, we can estimate the rates at which the ellipticities of the orbits of binary stars ought to increase, and this in turn makes it possible to estimate the ages of actual stars. It is a mere problem of dynamics, and the answer comes out in millions of millions of years.

We can now estimate the total amount of radiation which must have been emitted by particular stars in the millions of millions of years they have existed; and, except in the case of the youngest stars of all, the total mass of this radiation is found to be far greater than the present mass of the star. We obtain the mass of the star at its birth by adding the mass of all this radiation to that of the matter now remaining in the star. Thus its mass at birth must have been far greater than now. But, as a star's mass at any instant consists almost wholly of the mass of the matter of which it is composed, we see that the greater part of the matter contained in the original star has ceased to exist as matter; it has been annihilated and transformed into radiation which the star has radiated away into space. So far back as 1904 (NATURE, 70, p. 101) I put out the suggestion that energy might be created by the annihilation of matter; it now appears that this process must in actual truth be the source of the energy emitted by the sun and stars. Throughout a star's interior, electrons and

protons must at intervals fall into one another and mutually destroy one another, the energy of their fall being set free as radiation.

The energy of this fall is enormous, being sufficient to set both the masses involved into motion with a velocity of 0·866 times that of light. In no other way can a given mass of matter be made to yield energy of amount comparable with this; for example, whereas the ordinary combustion of a ton of coal provides energy enough to drive an express locomotive for an hour, the annihilation of a ton of coal would provide enough energy for all the heating, lighting, power, and transport in Great Britain for a century.

Each proton or atom, as it is annihilated, makes a splash of radiant energy which travels through the star until, after innumerable absorptions and re-emissions, it reaches the star's surface and wanders off into space. Each splash is similar to the splashes produced by radioactive material in the spinthariscope, except for being many thousands of times more powerful. The great energy of the splashes is to some extent counterbalanced by their rarity. In the sun, for example, only about one atom in every 10^{17} annihilates itself each hour. A cubic centimetre of the sun's mass contains, let us say, 10^{22} atoms, and of these about 100,000 are annihilated every hour. The energy produced in a cubic centimetre of the sun's mass is thus not very great, averaging about 9400 ergs or 0·00022 calories per hour; the enormous flow of energy from the sun's surface results from the fact that all the energy produced in a cone 433,000 miles in depth has to stream out through the mouth of this cone.

Such, in brief, is the mechanism by which stellar energy is generated. The question immediately before us is whether this generation of energy proceeds more merrily, whether the electrons and protons fall into one another more frequently, when the stellar matter is in a state of high temperature and high density.

It is a matter of direct observation that ordinary radioactive processes cannot be either inhibited or intensified by such temperatures as are available in the laboratory; the quantum theory provides the reason. Einstein has shown how a sub-atomic generation of radiation can occur in either of two ways, spontaneously or through the stimulus of incident radiation, and it is easy to calculate the temperature at which the second process becomes operative. It is found that the quantum of radiation at this temperature must have energy equal to the energy set free by the sub-atomic change in question. The temperature necessary to expedite the disintegration of uranium is in this way found to be of the order of 120,000 million degrees, and it at once becomes clear why warming up uranium in the laboratory cannot speed up its disintegration. A similar calculation shows that the temperature necessary to influence the rate of sub-atomic annihilation of matter is of the order of 7,500,000 million degrees. It may be argued that a lower temperature, although not adequate to bring about the actual annihilation of matter, might set up sub-atomic processes of adequate intensity. This is true as regards a star's momentary radiation, but such processes cannot provide an adequate duration for the radiation. All processes which are affected by temperatures of less than about 7,500,000 million degrees leave the total number of electrons and the total number of protons in a star unaltered, whereas the whole evidence of astronomy is that the number of electrons and protons in a star must continually decrease.

With this figure before us, it is clear that the comparatively feeble stellar temperatures of less than a thousand million degrees must be quite inoperative in regard to the main generation of stellar energy; indeed the heat of the hottest of stellar interiors can have no more influence on the rate of annihilation of matter than a warm summer's day has on the rate of disintegration of uranium. Thus it seems abundantly clear that what is annihilating the matter of the stars is neither heat nor cold, neither high density nor low, but merely the passage of time.

These considerations notwithstanding, it has been suggested by Russell (NATURE, Aug. 8, 1925), whose ideas were afterwards adopted by Eddington (NATURE, May 1, 1926), that the annihilation of matter (which they agree to be the ultimate source of stellar radiation) may be produced by the raising of ordinary matter to a critical temperature of some 30 or 40 million degrees. Russell suggests that matter is, broadly speaking, inert until it reaches this critical temperature, when an unlimited transformation of matter into radiation suddenly takes place.

In addition to running foul of the physical principles just explained, this suggestion encounters the difficulty that the generation of energy it provides would not only be unlimited but also illimitable; when once it began there would be no stopping it. The proposal of Russell and Eddington would, in effect, make matter thermodynamically unstable at stellar temperatures by endowing it with the properties of an explosive at its flash-point. When once stellar matter reached its flash-point, its resulting annihilation would generate so much heat that the adjacent matter would also in turn be raised to the flash-point, and the whole star would almost instantaneously explode into radiation. The sky would show no steady star-light, but merely a succession of apparitions of novæ of the most terrifying kind, as the various stars reached their flash-points and 'popped off' in turn. In spite of the astronomical

eminence of its father and stepfather, I, for one, find it impossible to accept a hypothesis which is not only contrary to the general principles of physics, but also against which the very stars fight in their courses.

A general mathematical discussion of the stability problem shows that a star built of matter the rate of generation of energy of which is absolutely unaffected by changes of temperature and density, will be dynamically stable. But such a star, although stable, has not much stability to spare. If we change the properties of our stellar matter in the sense of making an increase of temperature increase the rate of generation of energy, we lessen the already small margin of stability. Any substantial step in this direction would render the star dynamically unstable.

Combining this purely dynamical result with the physical principles already explained, it becomes clear that we may, to a good first approximation at least, suppose that an increase in the temperature of stellar matter produces no increase at all in its rate of generation of energy.

Stellar radiation must either originate in types of matter known to us on earth or else in other and unknown types. When once it is accepted that high temperature and density can do nothing to accelerate the generation of radiation by ordinary matter, it becomes clear that stellar radiation cannot originate in types of matter known to us on earth. Other types of matter must exist and, unless physics and chemistry have gone very far astray in recent years, these other types can only be elements of higher atomic weight than uranium. The significance of the calculation which showed that stellar atomic weights are, in the main, higher than that of uranium now becomes apparent.

Recapitulation and Interpretation of Results.

We have now reached the conclusion, by three distinct paths, that the atomic weights of stellar atoms must in the main be higher than that of uranium:

(1) By direct calculations from Kramers' formula.

(2) From the consideration that the atoms near the centre of a star must be substantially heavier than those near its surface.

(3) From the consideration that atoms of atomic weight less than uranium, no matter how much they were heated or compressed, could not provide the intense and lasting radiation emitted by the stars.

The atomic weights of stellar atoms are not only found to be higher than that of uranium, but also they vary systematically from star to star. In brief, the youngest stars are found to have the highest atomic weights, and with this clue all the pieces of the puzzle are found to fit together.

We have to suppose that matter in its earliest state consists of a mixture of elements of different atomic weights, those elements the atomic weights of which are highest having the greatest capacity for the spontaneous generation of radiation by annihilating themselves, and, in consequence, having the shortest lives. These elements will be the first to disappear as the star ages, their disappearance reducing not only the mean atomic weight in the star but also the mean rate of radiation per unit mass, since these heavy elements are the most energetic radiators. Just as, on the coast, the hardest rocks survive for longest the disintegrating action of the sea, so in a star the lightest elements survive for longest the disintegrating action of time, so that ultimately the star contains only the lightest elements of all and so has lost all radiating power. Our terrestrial elements have so little capacity for spontaneous transformation that they may properly be described as 'permanent.' Calculation shows that if they underwent any appreciable transformation in periods comparable with the life of a star (say 10^{13} years) the spontaneous generation of heat by the earth's mass would make the earth too hot for human habitation. The radioactive elements are of course an exception; they probably represent the last surviving vestiges of more vigorous primeval matter, and so form a bridge between the inert permanent elements and the heavier and shorter-lived elements of the stars.

An interesting question is whether the heavy atoms change into radiation instantaneously, or only through successive stages of transformation. Astronomical evidence makes it fairly certain that the most massive stars contain more atoms than our sun, there being a wider range in the weights of the stars than in the atomic weights of their atoms. As these stars must in time become reduced to the mass of our sun, the process of evolution clearly calls for an actual annihilation of atoms; it is not enough to postulate a mere gradual decrease in the atomic weight of each atom until it ends as a permanent atom. Radioactivity suggests that this latter process may also occur, but the evidence of astronomy is that it is at best a subsidiary process.

The number of 'permanent' atoms in a massive star such as Antares or V Puppis cannot undergo any perceptible diminution in the next 10^{13} years, so that they must all survive in the final star of mass perhaps only a fiftieth of that of the present star. Thus some ninety-eight per cent. of the present masses of these stars must consist of non-permanent atoms. To put it in another way, the present mass of a star such as Antares or V Puppis must consist, as regards 98 per cent., of atoms which are destined to change into radiation, and as regards only 2 per cent., of atoms which cannot change into radiation. Clearly the primary

matter of the universe must be of non-permanent type; our terrestrial atoms are a mere residue of non-transformable ashes. Like the animalculæ of the raindrop looking out on to Niagara, we discern that our physics and chemistry are only the fringes of far-reaching sciences; beyond the seashore we have explored in our laboratories lies the ocean the existence of which we are only just beginning to suspect.

We are thus led to picture the youngest stars as formed of matter practically all of which is unknown on earth, being of atomic weight higher than that of uranium. This possesses the capacity of annihilating itself spontaneously, the energy produced in the process being set free as radiation. Its rate of generation of energy, as estimated from the luminosities of the youngest stars, is of the order of 1000 ergs per gram per second. As the annihilation of 1 gram of matter produces 9×10^{20} ergs of energy, the matter must have a 'period of decay' of 9×10^{17} seconds, or about 30,000 million years. As the star ages, and only less transformable matter remains, the period of decay is correspondingly lengthened. The matter in the sun, radiating 2 ergs per gram per second, must have a period of decay of 15,000,000 million years. It is these periods of decay which determine the rates of evolution and length of life of the stars. Broadly speaking, a star lasts as long as the atoms of which it is composed, and the lives of these atoms are constants of Nature.

We notice that the periods of decay of stellar atoms are long compared with the periods of ordinary radioactive decay, suggesting that the radioactive elements are mere transitory formations in the evolution of the elements.

The Critical Central Temperature.

A group of stars selected for having approximately equal masses—as, for example, the sun, Procyon, and the two components of α Centauri—might be expected *a priori* to have very different rates of generation of energy, with the result that the stars would have very different surface temperatures and also very different central temperatures. Indeed, on first approaching the question, the whole range of temperatures from zero to infinity would seem to be open for each of these quantities. Yet in actual fact the surface temperatures of the four stars mentioned, as also of all stars of the same mass, lie within the narrow range between 3700 and 8300 degrees; their central temperatures probably lie within the range from 15 to 100 million degrees. For stars of other masses the limits are different, and are substantially wider for stars of great mass. But the stars of any definite mass always show a definite upper limit of temperature, both for the surface temperature and for the central temperature. These limits are never exceeded, but the majority of stars of the particular mass in question seem to crowd towards them. The existence of one limit of course implies the existence of the other, and it seems likely that the limit to the central temperature is the more fundamental. Stars having the same mass as our sun never have central temperatures higher than 80 million degrees, while the majority have central temperatures not very far below 80 million degrees. For stars twenty times as massive as our sun, the corresponding limit is probably somewhere about 300 million degrees, while there are intermediate limits for stars of intermediate mass.

This is obviously one of the fundamental facts of physical astronomy: What does it mean? The normal event for a star like V Puppis, losing mass and capacity for generation of energy together, would be a gradual shrinkage of size accompanied by a steady increase of central temperature. What is it that checks this normal course of evolution so soon as the central temperature touches 300 million degrees?

I have recently suggested that the upper limit of temperature for any star is simply that at which its central atoms begin to be stripped nearly or entirely bare of electrons. This is merely a matter of simple calculation, but we have to suppose the atoms at the star's centre to have the high atomic weights which other considerations, as we have seen, assign to them. For example, a temperature of 300 million degrees suffices to strip the last electrons off atoms of atomic weight 300 or more. The fall in the critical central temperature as a star gets older and less massive is, on this view, a direct consequence of the decrease in the atomic weight of the stellar material, which occurs as the heaviest atoms gradually annihilate themselves.

It remains to explain why this temperature constitutes an upper limit, why a star cannot go on getting hotter after its innermost atoms are stripped bare of electrons. So far as I can see, only one answer is possible: the stripping of the electrons from an atom must remove its power of annihilating itself, and so must inhibit its capacity for generating radiation. The central atoms of the star now act precisely like the governor of a steam engine, regulating the generation of energy so that the central temperature is kept close to the critical temperature. If the star begins to get too hot, the central atoms become stripped bare of electrons, and so leave off generating energy. The star then begins to cool off, and as it does so the atoms reform and resume their generation of energy, again heating up the star. The mechanism provides a perfect thermostat, and it is easily shown that its action is stable. As a star ages the heavier atoms at its centre are the first to be transformed into radiation and so to

disappear; their place is taken by lighter atoms, and as a lower temperature suffices to strip these lighter atoms bare of electrons, the critical central temperature of the star falls.

An interesting confirmation of this hypothesis is provided by the components of newly formed binaries. These have the high energy-generating capacity per unit mass of young stars, associated with the small mass appropriate to much older stars. Clearly the 'governor' action ought to be particularly active in checking the generation of energy in these stars, so that they ought to have central temperatures close up to the maximum for their mass. This is in actual fact found to be the case.

All the evidence at present available points to the annihilation of matter being a quantum phenomenon; possibly it represents nothing more than the spontaneous drop of an electron to a zero-quantum orbit. This would suggest an explanation of why bare nuclei and free electrons should be immune from annihilation, and hence why atoms stripped bare of electrons cannot generate energy.

Highly-penetrating Radiation.

If our earth exhibits only one end of the chain of chemical elements, where shall we look for the other end? Moving backwards along the evolutionary sequence we come to younger and yet younger stars, containing elements of higher and higher atomic weight. Passing beyond the stars altogether we come to the nebulæ; here we ought to find the elements of highest atomic weight of all, and the matter of greatest radiating capacity.

Visually the nebulæ are extremely faint objects; their emission of visual radiation per unit mass is only about equal to that of our sun. There is, however, an essential difference between the radiation generated in the stars and that generated in the nebulæ. Radiation, when first generated, must have enormous penetrating power; the simultaneous annihilation of a single electron and proton produces radiation of wave-length only $1{\cdot}3 \times 10^{-13}$ cm. The high penetrating power of this short wave-length radiation, nevertheless, only suffices to carry it through a small fraction of the radius of a star, and successive absorptions and re-emissions soften it, by a sort of generalised Compton effect, until it finally emerges from the surface of the star as ordinary temperature radiation. The density of the nebulæ is, however, so much lower than that of the stars that similar radiation, when generated inside a nebula, passes almost unchecked directly into outer space. Here and there the radiation may devastate isolated atoms in its passage, ejecting a few million-volt electrons in the process, but the majority of it will pass on unhindered until it meets a medium of substantial absorbing powers. Thus we should expect the atmospheres of the stars, sun, and earth, and even the solid body of the earth, to be under continual bombardment by highly-penetrating radiation of nebular origin.

Such radiation has been detected in the earth's atmosphere by Kolhörster, Millikan, and many others, who are satisfied that it is of extra-terrestrial origin. If it originated in the stars, the amount received would depend largely upon the position of the sun. As it does not, the radiation must originate in nebulæ or cosmic masses other than stars. Quite recently (Nature, October 9, 1926), Kolhörster and von Salis have found that its intensity varies with the position of cosmic masses, in a way which indicates that the radiation is received largely from regions near the Milky Way, especially the regions of Andromeda and Hercules.

I have calculated that the total amount of highly-penetrating radiation actually received is of the order of twice that which ought to be received from the Andromeda nebula alone (Nature, December 12, 1925) if this consisted solely of matter of the same radiating power as the very youngest of the stars. Clearly the total amount of radiation which is observed to be received on earth is of the right order of magnitude; it is, moreover, so large that it is difficult to imagine any possible origin for it other than that just mentioned. Its penetrating power appears to be rather less than might have been expected if it originated in the actual annihilation of electrons and protons, but I do not think the difficulty, if it exists, is insuperable. Quite recently Rosseland (*Astro. Jour.*, May 1926) has suggested that bombardment by this radiation may be the cause of the observed bright lines in stellar spectra; I had previously (Nature, December 12, 1925) suggested a similar origin for the luminosity of the irregular nebulæ.

There is a temptation to try to probe still further into the physics of the nebulæ, to try to understand the properties of matter in its still earlier forms, perhaps even to get a glimpse of it in the actual process of creation. But to yield to this temptation would carry us too far into the realms of conjecture and speculation. So far the course of our argument has not depended on either conjecture or speculation. Where there has appeared at first to be a choice of ways, all ways except one have proved on further examination to be prohibited either by observational knowledge or by well-established principles of physics or dynamics: there has never been any real choice. For this reason the conclusions we have reached, although certainly novel and perhaps unexpected, appear to me to be, in their main lines, inevitable; I can see no means of escape.

Life and the Universe.

A general survey of the results obtained by cosmical physics has suggested that terrestrial laboratory physics is a mere tail-end of the general science of physics. The primary physical process of the universe is the conversion of matter into radiation, a process which did not come within our terrestrial purview at all until 1904. The primary matter of the universe consists of highly dissociated atoms, a state of matter which, again, was not contemplated before 1917. The primary radiation of the universe is not visible light, but short-wave radiation of a hardness which would have seemed incredible at the beginning of the present century. Indeed, our whole knowledge of the really fundamental physical conditions of the universe in which we live is a growth of the last quarter of a century.

The simple explanation of this situation is to be found in the fact that life, naturally enough, begins its exploration of Nature by studying the conditions which immediately surround it; the study of the general conditions of the universe as a whole is a far more difficult task which life on this planet is only now approaching. Now the physical conditions under which life is possible form only a tiny fraction of the range of physical conditions which prevail in the universe as a whole. The very concept of life implies duration in time; there can be no life where the atoms change their make-up millions of times a second and no pair of atoms can ever become joined together. It also implies a certain mobility in space, and these two implications restrict life to the small range of physical conditions in which the liquid state is possible. Our survey of the universe has shown how small this range is in comparison with the range of the whole universe. Primeval matter must go on transforming itself into radiation for millions of millions of years to produce an infinitesimal amount of the inert ash on which life can exist. Even then, this residue of ash must not be too hot or too cold, or life will be impossible. It is difficult to imagine life of any high order except on planets warmed by a sun, and even after a star has lived its life of millions of millions of years, the chance, so far as we can calculate it, is still about a hundred thousand to one against its being a sun surrounded by planets. In every respect—space, time, physical conditions—life is limited to an almost inconceivably small corner of the universe.

What, then, is life? Is it the final climax towards which the whole creation moves, for which the millions of millions of years of transformation of matter in uninhabited stars and nebulæ, and of waste radiation into desert space, have been only an incredibly extravagant preparation? Or is it a mere accidental and possibly quite unimportant by-product of natural processes, which have some other and more stupendous end in view? Or, to glance at a still more modest line of thought, is it of the nature of a disease which affects matter in its old age, when it has lost the high temperature and capacity for generating high-frequency radiation with which younger and more vigorous matter would at once destroy life? Or, throwing humility aside, is it the only reality, which creates, instead of being created by, the colossal masses of the stars and nebulæ and the almost inconceivably long vistas of astronomical time? There are too many ways even to enumerate of interpreting the conclusions we have reached; I do not, however, think there is any one way of evading them.

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Retrospect.

By Sir J. J. THOMSON, O.M., F.R.S.

THE editor has asked me for some personal recollections of the Cavendish Laboratory, and I feel it would be churlish to refuse, though it is difficult to condense the happenings of more than forty years into a reasonable space, and still more difficult to avoid being egotistical. My connexion with the laboratory did not begin until I took my degree in January 1880. Before coming up to Cambridge I had, at the Owens College, under the inspiration of Balfour Stewart, become keenly interested in experimental physics and had done a small piece of research which was published just fifty years ago in the *Proceedings of the Royal Society*. The work for the Mathematical Tripos was, however, too heavy to leave time for experiments in the laboratory. As soon as the Tripos was over I started work at the laboratory, where Lord Rayleigh had just been made professor, and at his suggestion I began using a method, which he designed, an investigation on the ratio of the electrostatic to the electromagnetic unit of electricity. I made also theoretical investigations on applications of generalised dynamics to physics and chemistry, on vortex motion, and on the effects produced by the motion of electrical charges. There was great activity at the laboratory during Lord Rayleigh's time; Glazebrook and Shaw organised courses of instruction in practical physics; and Mrs. Sidgwick, Schuster, Glazebrook, Shaw, Poynting, E. B. Sargant, McConnel, J. A. Fleming, Hart, Miss Harland, and J. H. Middleton were working at various researches.

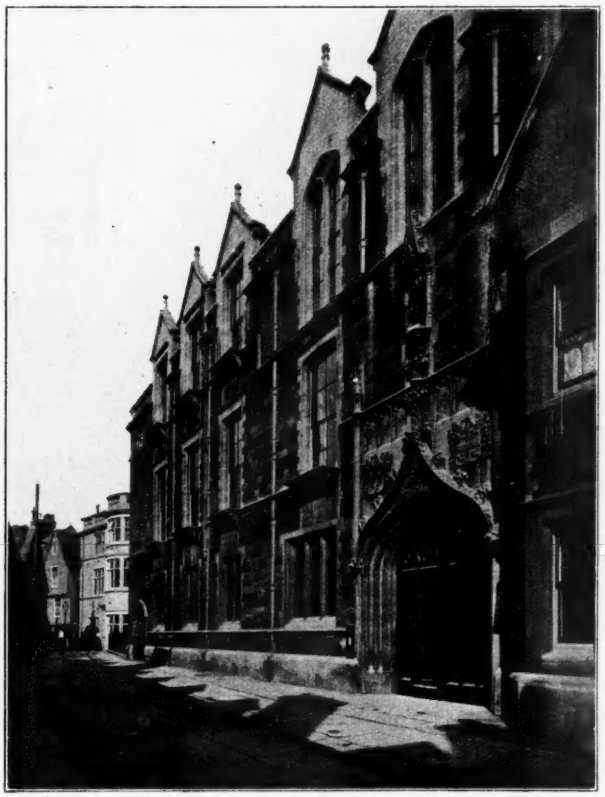


FIG. 1.—General view of part of the Cavendish Laboratory from Free School Lane. The new Rayleigh Wing is seen in the distance.

At the end of 1884, Lord Rayleigh resigned the professorship, and to my great surprise, and I think to that of the rest of the University, I was appointed to succeed him. I heard at the time that one well-known college tutor had expressed the opinion that things had come to a pretty pass in the University when mere boys were made professors. My want of experience was made less harmful than it might have been by the kindness of Glazebrook and Shaw, who continued to take charge of the teaching in practical physics. Immediately after my election to the professorship I began, in collaboration with my old friend Richard Threlfall, one of the most skilful experimenters I have ever met, some experiments on the passage of electricity through gases, and since then I think there has never been a time at which I have not had some experiments in hand on this subject. I had come to the conclusion that whenever a gas conducts electricity, some of its

molecules must split up and that it is these particles which carry the electricity. My idea at that time was that the molecule was split up into two atoms, one of which was positively, the other negatively, electrified, and my first experiments were intended to test this idea. It was not until the beginning of 1897 that I discovered that the decomposition of the molecules was of quite a different type from ordinary atomic dissociation. I found then that one of the bodies into which the molecules split up, the one carrying the negative charge, is something totally different from an atom, and has a mass less than a thousandth part of the smallest atom known. I was fortunate enough soon after becoming professor to be able to persuade H. F. Newall, now professor of astrophysics in the University, to come and help in the work of the laboratory, and for some time we worked in collaboration on various researches.

In 1887 a great scientific discovery was made which appealed especially to every one connected with the laboratory: I allude to the detection by Hertz of electrical waves. The existence of these waves is a necessary consequence of Maxwell's electromagnetic theory, but though numerous attempts to detect them had been made, some of them at the Cavendish Laboratory, they had all failed owing to the want of a sufficiently delicate instrument to detect their presence. Hertz, by observing the variations in intensity of the minute sparks which pass between conductors near together when electrical waves pass over them, was able to demonstrate the existence of these waves. The younger school of mathematical physicists at Cambridge, who had been brought up on Maxwell's "Electricity and Magnetism," received Hertz's discovery with the welcome which might be expected from those who found their most cherished convictions confirmed by a series of most beautiful experiments; it established instead of revolutionising our ideas. I well remember the enthusiasm of the undergraduates when I repeated Hertz's experiments in my lectures on electricity and magnetism in 1888. The enthusiasm spread to all the workers in the laboratory, and soon experiments on electric waves were going on all over the building.

Among the Cambridge men doing researches in the laboratory up to 1895—the year of the admission of research students—were, in addition to those already mentioned, Cassie; W. H. Bragg for a short time; Wilberforce, who was a demonstrator until he became professor of physics at Liverpool; Chree, who became superintendent of Kew Observatory; H. L. Callendar, now professor at the Imperial College of Science, who was elected to a fellowship at Trinity College for natural science after having taken first classes in both classics and mathematics, and began at the Cavendish Laboratory those researches on the effect of temperature on the resistance of metals which have revolutionised thermometry; T. C. Fitzpatrick, now president of Queen's College, who for more than twenty years had the charge of the classes to medical students, and in 1904 presented to the laboratory apparatus for producing liquid air—this gave us the means of producing high vacua, and without it many of the most important researches would have been impossible; G. F. C. Searle, to whose devotion and enthusiasm the laboratory owes more than I can express, and whose fertility and ingenuity in designing instructive and ingenious experiments for demonstration in practical physics remains quite unabated; W. C. D. Whetham, later a tutor of Trinity College, who worked at problems on the properties of solution; Sidney Skinner, now principal of the South-Western Polytechnic, who was for many years a demonstrator; J. W. Capstick, until he became junior bursar at Trinity College; J. B. Peace; Alexander Anderson; C. E. Ashford; Miss Freund, and Miss Klaassen. Among our guests from other universities were Sir Oliver Lodge, who worked for one long vacation in collaboration with Glazebrook on the determination of v; A. M. Worthington, who made an important investigation on the tensile strength of liquids; while H. F. Reid, of Johns Hopkins, Baltimore, Olearski, from Lemberg, and Natanson, from Cracow, were the precursors of the many distinguished teachers from foreign universities who have honoured us by working in the laboratory.

The year 1895 was an epoch in the history of the laboratory, as then the University decided to admit as research students graduates of other universities whether at home or abroad, and in certain cases students who are not graduates, provided they satisfy the authorities that they are qualified to do research. In October 1895, the first two research students—Rutherford, from New Zealand, and Townsend from Trinity College, Dublin—arrived within a few hours of each other. Did a new system ever have such a good beginning? Rutherford began by working at wireless telegraphy, using a detector which he had invented before leaving New Zealand: it was so successful that the laboratory held for a time the record for long-distance telegraphy, communications being established between the laboratory and the observatory nearly two miles away. Townsend made a very important investigation on the magnetic properties of iron when in chemical combination.

It was fortunate that the new regulations, which brought to the laboratory a large influx of students bent upon research, came just at the time when the discovery of the Röntgen rays gave us a very powerful method of investigating the mechanism of the con-

duction of electricity through gases. A great many researches had already been made in the laboratory on this subject, and our studies had shown the fundamental importance of certain investigations which were almost impracticable with the means then at our command. The Röntgen rays gave us a means more amenable to purposes of investigation than those hitherto available and made many investigations possible which had before been hopelessly difficult. The investigation of the electrical properties of gases under the action of Röntgen rays soon engaged the attention of many workers in the laboratory. Rutherford, Townsend, McClelland, Langevin, who had come to us from Paris, were all hard at work. C. T. R. Wilson began those researches on condensation which, *ohne Rast ohne Hast*, he has pursued ever since, obtaining results of continually increasing importance for the development of modern physics.

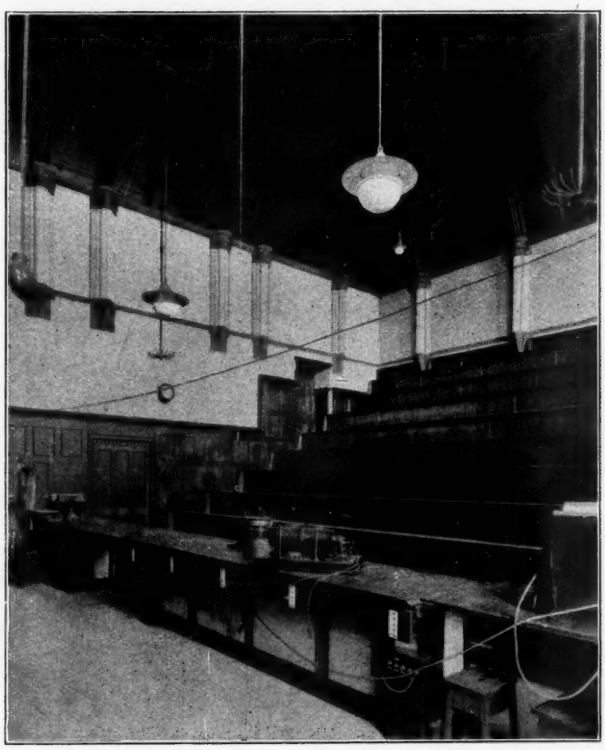


Fig. 2.—Lecture room of the Cavendish Laboratory, designed by Clerk Maxwell. Reproduced from a photograph kindly supplied by Mr. Rollo Appleyard, International Standard Electric Corporation.

It is not, I think, claiming too much to say that these researches played a great part in establishing the theory now universally accepted that the electricity is carried through gases by particles charged with electricity. The properties of these particles were studied in great detail at the laboratory, and they seemed to indicate that they were at least as large as the atoms of the gas in which they were produced. Some experiments which I made at the beginning of 1897 led, however, to the discovery of particles of quite a different order of magnitude. The cathode rays, which proceed from the cathode when an electrical discharge passes through a gas at low pressure, were discovered by Hittorf, and many experiments had been made on them by Goldstein, Varley, and Crookes.

Two widely different views were, however, held as to the nature of these rays. The majority of the German physicists maintained that they were analogous to electric waves, and urged in support that they could pass through thin sheets of metal. The English physicists held that they were charged particles, and brought forward the argument that when the rays entered an insulated vessel they communicated to it a charge of negative electricity. I had for a long time been convinced that these rays were charged particles, but it was some time before I had any suspicion that they were anything but charged atoms. My first doubts as to this being the case arose when I measured the deflexions of the rays by a magnet, for these were far greater than I could account for on any hypothesis that seemed at all reasonable if the particles had a mass approaching even that of the hydrogen atom—the smallest mass then known. I made a series of measurements of the velocity of these particles and the ratio of their mass to their charge. These all showed that the ratio of mass to charge for the cathode particles was less than one-thousandth part of the same ratio for the atom of hydrogen and its electrolytic charge. Thus, unless the electric charge on the cathode ray were at least a thousand-fold that on the electrolytic ion, the mass of the cathode ray particles must be less than that of an atom of hydrogen. Methods were devised by which the charge on these particles could be measured, and it was found to be the same as the electrolytic charge. I could see no escape from the conclusion that in the cathode rays there were particles far more minute than any hitherto recognised. I think the first announcement of this result was made at a Friday evening meeting of the Royal Institution on April 30, 1897.

Later investigations have shown that these particles—the electrons as they are now called—are always of

the same kind and form a part of every kind of matter. The number of electrons in the atoms of the different elements was determined by applying a theory of the scattering of these rays by electrons which I had worked out from the valuable measurements by Barkla on the amount of radiation scattered by different gases, and it was found that this number was proportional to the atomic weight of the element and was of the same order as that number. This led me to construct a theory of the structure of the atom which had special reference to the chemical properties of the element, for it was shown that such a structure would give a variation of the chemical properties with the atomic weight quite similar to that expressed by Mendeléef's law. Another excursion I made into chemistry was to use the magnetic and electric deflexions of the positive rays to determine the chemical composition of the gas in which they were generated. It proved a useful method, and Dr. Aston has modified and improved it, so that it is now one of extraordinary delicacy and accuracy.

The number of those engaged in research grew very rapidly, and for many years before the War there were always thirty or more researches going on in the laboratory. This put a great strain upon the apparatus and upon the workshop. We have been very fortunate in our chief assistants, and the laboratory owes much to Mr. Sinclair, Mr. Bartlett, and Mr. Pye, and especially to Mr. Lincoln, who has been chief assistant since 1899. Personally, I am greatly indebted to the skill and care of Mr. E. Everett, who has been my private assistant for nearly forty years, and has given me most valuable and able assistance in my investigations.

It is impossible here to mention individually the research workers between 1895 and the beginning of the War: their number is too great. Suffice it to say that three of them have gained the Nobel prize, twenty-two have been elected fellows of the Royal Society, and more than fifty have become university professors of physics. When the War began the usual work of the laboratory stopped. The research workers went either to the front or to laboratories formed for developing and testing methods likely to be of use to the fighting services. Our own workshop was employed in making gauges, and all the research at the laboratory was war-work. Soon after the end of the War my duties as master of Trinity obliged me to resign the professorship. The University was fortunate enough to induce Sir Ernest Rutherford to take the post. At the same time, with great kindness and consideration, they gave me a position which enabled me to retain my connexion with the laboratory which for more than forty years had given me unrivalled opportunities for doing the work I liked best, which has brought me many cherished friends and has created rich memories of kind acts, good fellowship, and goodwill.

James Clerk Maxwell.

By Sir Joseph Larmor, F.R.S.

THE course of evolution of the career of a man of genius is always an interesting, and should be an instructive, study. In the case of Clerk Maxwell the materials are ample, thanks to the care of his two biographers, one of them a classical scholar with Greek predilections, the other his intimate and sympathetic scientific assistant. Their account leaves an impression of the absence of any formal education outside his home life. His father was his chief early friend; his real intellectual initiation was identical with his amusements, mainly concerned with dynamical contrivances such as spinning tops, and extending into explorations in practical hydrodynamics by feats of swimming in the bath. Perhaps the Scottish atmosphere of practical engineering, fanned into interests of pure science by men such as Robison, Kelvin, Rankine, explained the origin of this mental bent; but it does not account for the literary grace and charm of his writing.

Another main formative influence was the Scottish Calvinist theology, in its more humane and devotional aspect, supported by intimate conversance with the phrasing and poetry of the English Bible, which has so often been the well-spring of distinction in literary expression. This Puritan trend, while it treasured the historical formularies of the Scottish Church, was in him far from sectional; it ramified into enjoyment of literature, such as the poetry of Milton, and also the descriptive and evolutional poetry of Tennyson, whose influence was still dominant in the Cambridge of his undergraduate time.

Another phase of Maxwell's thought was made public in the biography, in some occasional papers of metaphysical and religious import, which had been read to a private Cambridge society. At the time of publication, as one remembers, the interest of them was regarded as mainly personal, being outside the analytic severity of the British psychology of that period. But they were discovered by Prof. Höffding of Copenhagen, and have come back to us, in the English translation of his works, as among the competent pronouncements on the philosophy of Nature.

It is Maxwell's main achievement that he unified physical science, by connecting light and radiation

with electricity so as to form one interlocked systematic whole. This advance made the Cavendish Laboratory, from its foundation in 1872, the focus of electrical pure science which it has remained ever since. It must have been remarked by students of his writings how abruptly he left off the development of this electrodynamic and optical theme in the last six years of his life, after the publication of the treatise on "Electricity and Magnetism" early in 1873. For example, no question ever occurred as to the dynamical riddle, then prominent in physical speculation, which was presented by the Fresnel laws of refraction of optical waves: though when Helmholtz, and Lorentz, and J. J. Thomson entered into the study of his theory, they at once recognised independently that the form, at any rate, of these laws was immediately involved, while FitzGerald, going deeper, had already connected the electric theory intimately with the entire *corpus* of the optical theories of MacCullagh.

One reason may have been Maxwell's preoccupation with fundamental objections of fact, operating against his views. His dynamical mentor, Lord Kelvin, to whom he largely owed the impulse to the elucidation through kinematic models of deep-seated physical phenomena, had here failed him, and to the end of his days held to the view that the foundations of the electric theory of light were unintelligible, by which he seems to have meant not sufficiently disentangled dynamically, his own fundamental developments on latent momenta notwithstanding. The wide discrepancy between the index of refraction of a substance and the square root of its dielectric inductive constant was such a threshold difficulty of fact, and loomed important in those days when the influence of dispersion was not very fully recognised. The first fundamental service of the illustrious Boltzmann to the theory of his master in research was the measurement of the dielectric constants of gases, resulting in confirmation of Maxwell's law for such simple forms of matter: while the experimental work of Hopkinson on organic compound substances also brought out significant correlations.

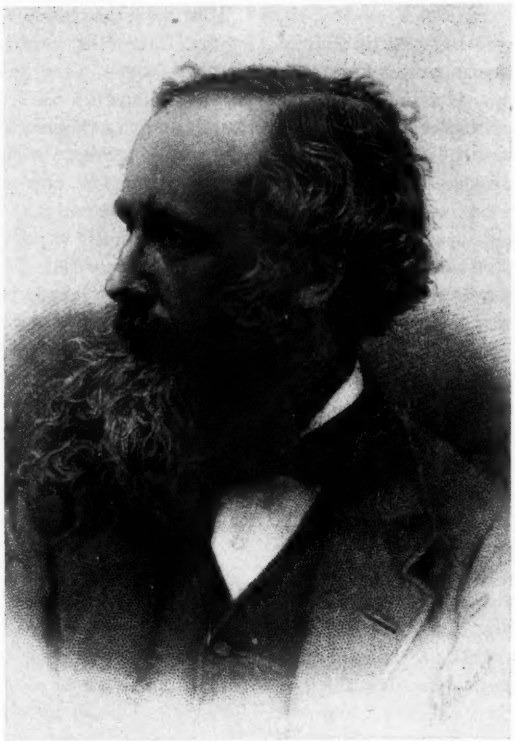


FIG. 3.—JAMES CLERK MAXWELL, Director 1871–79.

Curiously enough, it is now recognised universally, on the initiative of the late Lord Rayleigh, that Maxwell himself had been the pioneer in clear-cut dynamical atomic views of dispersion. He had formulated a fully illustrative scheme, in the guise of a rather detailed examination question in the Cambridge Mathematical Tripos of 1869. This must have implied that the floating dynamical instincts of that time were adequate, at any rate in the view of himself and his co-examiners, for a rapid apprehension and verification of the ideas involved; while the collections of Maxwell's tripos problems, that were treasured and utilised in Cambridge teaching, must have made them not unfamiliar to expert optical students.

There is a cognate question, why in his expositions Maxwell made so slight use of moving electric charges as the originators of the electrodynamic fields which, after the example of Faraday, were his main concern. For the atomic constitution of electricity had been fully established by Faraday's own laws of electrolysis, as Helmholtz afterwards emphasised. The idea of electrons acting on one another through a law of attraction at a distance had been long before placed by W. Weber at the foundation of the reasoned exposition of his fundamental electric measurements. Maxwell himself hovers around the phrase 'atoms of electricity.' He even sets in special prominence the remark of Gauss that what was needed was, above all, some notion of how influence was propagated in time between the sources of the electric manifestations. Gauss confesses that he had not been able to find it; while Maxwell implies that it now lies exposed in his equations of the electrodynamic field. Yet he did not pursue this path. Hertz found no difficulty, in 1887,

in determining, on Maxwell's principles, the radiation from a vibrating source such as an oscillating electron. Indeed FitzGerald had already, five years earlier, incidentally determined the nature and amount of the radiation from a rapidly alternating current circuit, that is, from a magnetic vibrator; and on the same occasion he pointed out that a spark from an electric accumulator, discharged through a small resistance, ought to produce electric waves as short as ten metres or less. He thus was well qualified to expound Hertz's epoch-making detection of the Maxwellian waves in free space, in an address to the British Association soon after it was announced.

In other respects also Maxwell had largely confined himself to the medium of electric propagation as a whole, as modified by its content of material atoms and ions in bulk. He had measured, incidentally, with the assistance of Hockin ("Treatise" § 798; more fully in the Memoir[1] of 1864), the transparency of metallic foils such as gold-leaf, and found that it implied effective electric conductance far smaller than the steady value as determined on a bridge. The natural reason for this discrepancy he indicated: also the reason why electrolytes are not opaque; but there he left it, for indeed the relevant experimental knowledge was as yet a blank, while other subjects were pressing on him. We may perhaps add to this the distractions involved in the creation of the Cavendish Laboratory, and the laborious editing and expansion of the Cavendish electrical manuscripts.

In the same context ("Treatise," § 792; more fully in the Memoir of 1864, § 107) he propounds very briefly his law of radiation pressure, now fundamental in physics and astronomy; he tries to estimate the magnitude of the pressure for sunlight, coming to grief in detail over the arithmetic. He perhaps reached the law more by physical instinct than by demonstration. If we imagine a limited train of electric waves reflected back at a perfect conductor, a current sheet is induced on it, and the Amperean force on that sheet constitutes a pressure of the train on the conductor. But the train was originally isolated in space, and will again be isolated with only its direction reversed; thus there is no other source for the pressure exerted by it, on the electrons of the current, but a change, here a reversal, of momentum associated somehow with the radiation. This is the idea developed directly in experiment by Poynting, that a train of radiation is a carrier of momentum. Maxwell associated the pressure with his quadratic stress, in origin purely formal, in the electrodynamic field. Close scrutiny, first in time by Lorentz, reveals, however, an outstanding motional part of the mechanical force which is not absorbed into that stress-tensor, significant though it was; and Poincaré and Abraham remarked that it was just such as could arise from a distribution of momentum in the field, thus without vitiating the stress-representation. The complete forcive on the material content of any region is then expressed as the resultant of this formal electromagnetic stress over its boundary, when there is such stress there, together with the rate of communication of this aethereal momentum throughout its volume: if there is no material content these will balance.

It is, then, the transfer of this postulated momentum of radiation that constitutes radiation pressure on material bodies. The transfer is effected, as above, through Amperean force on the current of electrons. Reversing the argument, radiation pressure, as experimentally confirmed by Lebedew, Nichols and Hull, Poynting, implies momentum in radiation; and its existence to the requisite amount implies a reality, of some kind scarcely yet fully explored, in the Maxwellian quadratic field-stress. An intimate knowledge of the structure of the electron which sustains the stress ought to involve both its own dynamical nature and the structure of the field in which it subsists.

It is not necessary to follow up Maxwell's share in the practical settlement of the scheme of electric units, the essential preliminary to the present electrotechnic age; in that Kelvin was the leader. But we may note his early memoir in determination of the nature of subjective colour, in which he verified with precision the surmise of Young, following on Newton, while Maxwell himself was followed by Helmholtz and by Rayleigh, that all the gorgeous play of colour that is a main glory of our natural world arises from combinations of only three independent elements.

It has been already remarked that Maxwell added little in later life to the essentials of the electric theory. The first edition of the "Treatise" had obviously been thrown hurriedly into the press, in a series of rather disjointed fragments which taxed the ingenuity of dynamical interpretation of his British disciples for years; some additions for the early chapters of a second edition were mainly developments of the usual standard knowledge. But he could still be drawn on the subject. Compare his answer in keen and humorous verse to Tait ("Life," p. 684 (1877), already however in "Treatise" (1873), § 577), who proposed apparently to repeat with a spinning excited dielectric disc Rowland's then recent experiment of the magnetic convection effect of a charged rotating disc, as was afterwards carried through by Röntgen. He wanted Tait to spin instead a copper coil with galvanometer in circuit, suddenly to arrest its motion, and note if

[1] The abstract of this memoir (*Roy. Soc. Proc.* **13**, pp. 531-536), accidentally omitted in the "Scientific Papers," is an interesting general exposition of the author's ideas, largely in line with modern points of view

there was any result; for that would give "the electric current's true direction," or alternatively let it "be your boast to prove . . . that there is no Electric Fluid." This test for sensible inertia of the electrons, as we would now say, has been carried through to success in America only the other day.

Plenty of electric discussion was, however, going on, especially in the years just after Maxwell's death. It was regarded as a great improvement when Heaviside and Hertz, nearly simultaneously, got rid of his vector potentials as being mere mathematical figments, though with him of heuristic dynamical origin; yet the two resulting circuital relations, as Kelvin called them, had already been formulated long before by Maxwell himself, as a concession to a demand for the essential outcome of his theory, in concise form freed from tentative dynamical implications. But these circuital equations are concerned only with the smoothed-out electrodynamic fields. Ultimate dynamical theory, going back to the sources of the field, has not yet been able to do without their vector potentials. Nowadays the circle has indeed gone round full tilt; we have been familiarised with the point of view that the electric and magnetic fields, so tangible in the world of engineering, are in theory only two partial aspects of one six-vector, itself the (Hamiltonian) gradient of a fourfold vector potential that alone is fundamental in Nature, as presented to our minds.

Maxwell's other main contribution to science, equally monumental, lay in the domain of the molecular kinetic theory of gases; it provided the more severe mathematical occupation of his later years. He had taken over the subject in his early days from Joule and Clausius and their predecessors, in the form of a rough *aperçu* of the phenomena of a crowd of independent moving molecules: he converted it into an exact theory, thereby creating the science of statistical dynamics which dominates modern molecular physics. The root principle of that science is Maxwell's law of distribution of velocity, or of other quality, in a multitude of molecules which has attained a steady state. As results there came to him exact dynamical theories of friction and diffusion and conduction of heat in gases, and analytical developments for rarefied gas arising out of the phenomena revealed by the Crookes radiometer. Here also Boltzmann was in readiness to follow up this train of research. From their memoirs in general statistical dynamics, the law of equipartition of energy among the various modes of molecular freedom stood out as a cardinal result. The problem of how the inadmissible consequences of this law are to be evaded has opened up new regions of physics, practical as well as speculative, which at present tend to dominate the whole field.

A characteristic illustration of his genius was the early enforcement of the averaging character of the processes of thermodynamics, by appeal to the possible achievement of ideal minute intelligences, named Maxwell's demons by Kelvin, who could by merely guiding or sifting interference upset the fundamental principles of that science; this arresting quip carried the new doctrine of the statistical character of natural law for molecular structures into regions of thought where abstract dynamical argument could scarcely have penetrated. His lucid expositions in formal thermodynamics need only be mentioned; in them he appeared mainly as the simplifier of the fundamental advances achieved in the American work of Willard Gibbs, the foundation of modern physical chemistry.

Maxwell spent his summers on his small estate of Glenlair in Galloway, among his own people, living as a Scottish laird. Doubtless it was there, among the solitudes of the hills, that illumination mainly came. In the autumn he usually attended the British Association, where, as one used to hear, his gaiety and humour were looked forward to as enhancing the value of the annual scientific discussions, then at their prime. The writer recalls that, returning to Cambridge as an undergraduate one October, a man of the type of a country farmer came into his compartment of the train at Dalbeattie, remained silent for a time, then remarked with emphasis, as something that concerned the world to know, to this effect: Clerk Maxwell has been taken away, mortally stricken; he will never come home again. He died in 1879 at only forty-eight years of age.

Lord Rayleigh.

By Sir Arthur Schuster, F.R.S.

MAXWELL'S health began to fail in the early part of 1879. Troubled by his wife's illness, which weighed heavily upon him, his accustomed good spirits had left him; but there were no signs that he himself was suffering from a mortal disease until his return from the summer holidays, when we were shocked to hear that he had only a few weeks to live. His death was a calamity which might have been fatal to the continued prosperity of the Cavendish Laboratory had Lord Rayleigh not consented to accept the professorship. His hesitation and the pressure put upon him by those who had the interest of the laboratory at heart, are set out in the 'Life' of the father written by the son.

After his election, Rayleigh lost no time in making

plans for the conduct of his new duties. I am sorry not to have preserved a letter I received from him asking whether I had any suggestions to make, as I had gained some experience in its working during the two years I had spent in the laboratory. Others received similar requests, but I doubt whether the new director of the laboratory received any useful hints on essential matters.

On his own initiative Rayleigh adopted a definite and novel policy, its essential point being the fostering of a spirit of community among the advanced students. To make a beginning, he desired to identify the laboratory as a whole with some research in which a combination of workers was necessary. The question of electrical units had, at that time, gained practical importance and the subject seemed suitable for a combined attack. There was the additional reason that Maxwell had been connected with the original determination of the Ohm fathered by the British Association, and that the principal piece of apparatus, the rotating coil, was preserved in the laboratory.

Fig 4.—Lord Rayleigh, Director 1879–84.

Different methods of procedure having led to values which differed more than was desirable, Rayleigh decided to repeat the former work, using the same coil, but paying more attention to what others might have considered minor details. The method by which the rotational velocity of the coil was kept constant was simple and effective, and the same applies to the very ingenious method devised to compare the frequency of the tuning-fork, to which the rotation was referred, with the rate of a standard pendulum clock. When sufficient experience had been gained with the original coil, a new one was constructed giving a result which was decidedly more accurate than any obtained up to that date. Rayleigh was disappointed in his original desire, no volunteers besides myself offering to help, until Mrs. Sidgwick joined us, and the later history of the research is well known.

I may, perhaps, here refer to a personal recollection which has always remained in my mind. During the early stages of the investigation of the Ohm, Lord Rayleigh made the remark to me that this was the first piece of work he had undertaken that required great accuracy of measurement, and he added that he was not at all sure that he would really like it. I replied that I felt sure that the satisfaction of getting another decimal place in a physical constant was a pleasure which would grow on him more and more. Later events, I think, justified my remark.

Persevering in his desire to form a kind of research community within the laboratory, Rayleigh introduced an innovation that may seem trivial, but has been effective in more than one laboratory since. A tea-interval was introduced, during which the different workers could meet and join in informal discussion on scientific problems. Tea was served in a room in which other experiments were kept going, and those who had the privilege of attending these informal scientific meetings will more especially remember the water jets breaking into drops, and the effects of electricity on the appearance presented by them. There were also the spinning colour discs in which we could compare our colour senses.

A good example of Rayleigh's method of work was shown by the little appliance made with card-board, sealing-wax, simple glass lenses, and a prism or two, by which he could determine the relative amount of red and green required by different persons to produce the yellow of a sodium flame. By comparing the results of different observers it was found that, while most persons agreed fairly well in their estimate, there are anomalies which run in families. Lord Balfour and two of his brothers, for example, required a con-

siderably larger proportion of green to get the yellow sensation. With a more elaborate apparatus designed by Rayleigh, I examined, on my return to Manchester, seventy-two persons and found among them four possessing the same peculiarity and one with an anomaly in the opposite direction. Among the four was a woman with two of her three sons. It is a curious coincidence that the colour sense of Clerk Maxwell was, and that of J. J. Thomson is, affected in the same way. An examination of the present director's sight is obviously indicated.

Though not belonging to the Cambridge period, Rayleigh's great work on the weighing of gases should be mentioned, because it was really planned at Cambridge. He frequently referred to the desirability of keeping a research going as a 'stand by,' that is, an investigation which presumably could be continued for a long time and dropped or taken up again as more urgent demands had to be satisfied or a slack time occurred. His choice fell on the weighing of gases, because he was always impressed by the probability of a unity of matter and the likelihood of the correctness in some form or other of Prout's law. Quotations from his address as president of Section A of the British Association giving his views on the subject will be found in the biography written by his son.

During Rayleigh's tenure of office at Cambridge a systematic and very successful instruction in laboratory work was introduced by Glazebrook and Shaw. For this and research purposes a substantial annual income was required, and Rayleigh raised a fund of 1500*l.*, to which he himself contributed one-third.

When he gave up the directorship of the Cavendish Laboratory, it was in a highly efficient state both as a teaching and research institution.

Sir J. J. Thomson, O.M., F.R.S.

By Sir Oliver Lodge, F.R.S.

HOW much less the world would know if the Cavendish Laboratory had never existed; and how diminished would be the glory even of that laboratory if Sir J. J. Thomson had not been one of its directors! We used to think of him as one of the younger physicists; but now that his seventieth birthday is being celebrated that notion must be given up, even by his seniors. But, whether young or old, we have all venerated him for his brilliant achievements.

The discovery of the electron and the foundation of the electrical theory of matter cannot, any more than other fundamental discoveries, be attributed to any single man: these great advances are the outcome of the work of at least a generation. To them Helmholtz, Crookes, Johnstone Stoney, Sir Joseph Larmor, and in the electrolytic stage even Faraday, have all contributed; and, doubtless, in mentioning some names I am omitting others. But researches into the phenomena connected with the discharge of electricity through gases have been Sir J. J. Thomson's special field; and, as Clerk Maxwell hinted would happen, that branch of inquiry has thrown great light upon the nature of electricity itself. Very few before our time can have supposed that electricity was discontinuous. Maxwell's equations and Cavendish's experiments either postulate or appear to demonstrate continuity and incompressibility. That electricity was a fluid, comparable in any respect to a gas, seemed like a popular superstition. The discovery of a discontinuity in Nature must always have notable consequences; and though we may be willing to grant that ultimately every atomic character will be resolved into a deeper-seated and more fundamental continuity, yet for a long time it will be the business of science to absorb and work out the consequences of every discontinuity that is revealed.

John Dalton was the earliest to emphasise the chemical discontinuity of matter. J. J. Thomson is likewise the first to emphasise effectually the atomic character of electricity. His work is a happy combination of experimental and mathematical ability. He arranges ingenious experiments to display and dissect the phenomena; and at the same time most skilfully applies dynamics to analyse those phenomena in an illuminating and metrical manner. In his hands the magnetic deflexion of cathode rays observed by Crookes, coupled with certain other experiments by C. T. R. Wilson and J. S. Townsend, sufficed to determine the mass and the speed of the electric particles; and when electric deflexion of cathode rays was combined with magnetic deflexion, the determination could be made in a particularly neat and convincing manner. So that when Sir J. J. Thomson gave an account of his researches throughout the years 1897 and 1898, before Section A of the British Association meeting at Dover in 1899 (in the presence of a number of Continental physicists, many of whom had come over that day from Boulogne), the whole world rose to the conviction that a new era had dawned in electrical science. A foundation-stone was then laid for the innumerable researches which have gone on during the present century in every laboratory and library of the world.

The facts are so well known now that it is needless to elaborate them: within thirty years they have

become almost ancient history. Naturally the renowned discoveries of Becquerel, Röntgen, and Madame Curie fell into line. The discovery of Zeeman, verifying that the radiating particle was not an atom but an electron, worked out initially by the genius of H. A. Lorentz, fell in also with a classical theory of radiation conforming to ideas promulgated by Hertz and Larmor and FitzGerald; though it is true that certain difficulties and puzzles afterwards arose, in which all physicists, including J. J. Thomson himself, are now deeply involved.

We used to think that Clerk Maxwell had explained to us the nature of light; and so no doubt to a great extent he had; but our theory of the emission and absorption of light has been singularly complicated by the action and reaction between electrons and radiation, to which O. W. Richardson, Barkla, and other experimenters, including Compton, have contributed so much information. Especially have both complication and illumination been enhanced by the discovery of that new discontinuity which now penetrates our treatment of every interaction between ether and matter—the outcome of singularly successful speculative reasoning by Max Planck at the beginning of the present century.

Photo] [*J. Palmer Clarke.*

FIG. 5.—SIR J. J. THOMSON, O.M., F.R.S., Director 1884–1919.

The accuracy with which both the charge and the mass of the electron are now known (largely through the ingenious measurements of Prof. Millikan), and the researches of spectroscopists all over the world, from Kayser and Runge, Schuster and Hicks, Balmer and Rydberg, down to Prof. A. Fowler—illuminated as they are by the discoveries of Rutherford and the brilliant constructions of Bohr—have raised an enduring monument, a sort of cathedral, glorifying the electrical theory of matter. To the completion of that splendid structure, the present generation, and probably many future generations, are contributing and will contribute. All this must be a great satisfaction to the Master of Trinity, who is still so happily engaged in his own researches that he has perhaps scarcely time to bethink himself of the beauty of the edifice which has been raised on his foundation-stone.

Of course J. J. Thomson has done many other things. At a very early stage he secured the Adams Prize for a theory of the behaviour of vortex rings, which may still one day be usefully drawn upon when the hydrodynamical structure of the ether, already begun long ago by Larmor, is seriously undertaken. Again, the electrical theory of matter would be nowhere if we did not know that an electric charge possessed inertia. The resolution of matter into energy, insisted on by the theory of relativity, has as one of its props that early paper of J. J. Thomson's in 1881 (forty-five years ago !), in which he calculated the extra mass conferred on a body by an electric charge; a suggestive idea to which both he and Heaviside contributed the further illuminating fact that that inertia would be increased by locomotion; a prediction which, when verified by Kaufmann, made an electrical or ether-field theory of matter inevitable.

Thomson did not limit himself to metrical determinations about negative corpuscles and cathode rays. He attacked also the positive or matter rays; and thereby developed a fundamental technique of positive-ray analysis, which speedily detected a variant of neon, and then in the hands of Aston defined atomic weights with altogether unexpected precision. This method, brilliantly and pertinaciously applied, has fully upheld the isotopic conception of Soddy (preceded as that was by the unorthodox speculation of Crookes at the Birmingham meeting of the British Association in 1896), and has thereby not only brought to our ken a host of new substances with similar chemical but different physical properties, but also has established in modified form the hypothesis of Prout, that atomic weights are really whole numbers, of which hydrogen is very nearly, though not quite, the common unit.

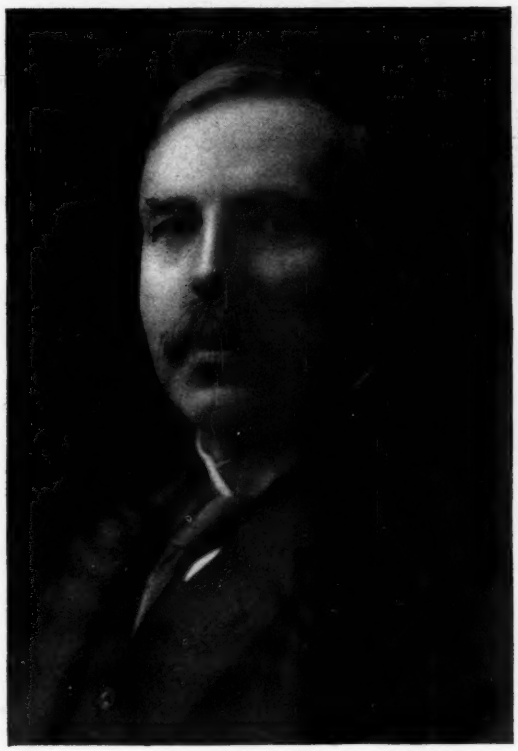
Readers who wish for a more detailed reminder of the steps which have led to these great advances may refer back to NATURE, vol. 91, p. 1, where Prof. Righi of Bologna contributes an article appreciating, from the Continental point of view, the work of Sir J. J. Thomson up to the date 1913.

Students and disciples all over the world could contribute far more details. His own son, the professor of physics at Aberdeen, is one of the brilliant products of the Cavendish Laboratory; and many prominent physicists, such as the present Lord Rayleigh, could testify with intimate knowledge of the work of that laboratory during Thomson's régime. They know the doubts and hesitations which had to be set at rest before the absolute uniformity of electronic charges could be confidently asserted. They know the persistent help given by Mr. Everett, his laboratory assistant for nearly forty years. They are acquainted with the incipient stages of many discoveries. But an older physicist esteems it a privilege to write this brief appreciation of the achievements of one who has worked with unexampled power in the borderland between chemistry and physics, who has introduced into that great science of chemistry revolutionary conceptions the end of which none of us can see, and who is still happily flourishing and active.

Sir Ernest Rutherford, O.M., P.R.S.

By Prof. NIELS BOHR, For.Mem.R.S., University, Copenhagen.

FOLLOWING the kind invitation of the editor to write a few words in appreciation of the work and influence of the present director of the Cavendish Laboratory, I presume that the readers of NATURE will not need any detailed exposition of his achievements. As, however, I am one of those who have had the good fortune to come into close personal and scientific contact with Sir Ernest Rutherford, it is a great pleasure to me to try to describe briefly how we, who are proud to count ourselves among his pupils, regard him.



Photo] *[J. Russell and Sons.*

FIG. 6.—SIR ERNEST RUTHERFORD, O.M., P.R.S., Director 1919– .

My own acquaintance dates from the period when Rutherford, after years of ardent and successful collaboration with Sir J. J. Thomson in the Cavendish Laboratory, had left Cambridge, and —after his stay at McGill, where his work on radioactive substances had established his fame—in Manchester had founded a school for investigations in radioactivity. This centre attracted young scientists from all parts of the world. In the spring of 1912, on my first visit to Manchester, the whole laboratory was stirred by one of the great discoveries which in so full a measure have been the fruits of Rutherford's endeavours. Rutherford himself and his pupils were eagerly occupied with tracing out the consequences of his new view of the nuclear structure of the atom. It would give only a poor impression of our trust in his judgment for me to say that nobody in his laboratory felt the slightest doubt about the correctness and fundamental importance of this view, although naturally it was much contested at that time. I remember being told by Hevesy soon after my arrival the story circulating in the laboratory of how Rutherford, shortly before his discovery, in a conversation with Moseley expressed the opinion that after all the troublesome investigations of the preceding years—during which he had such faithful assistance from Geiger — one would have had quite a good notion of the behaviour of an α-ray, were it not for the return of a minute number of these rays from a material surface exposed to an α-ray bombardment. This effect, though to all appearances insignificant, was disturbing to Rutherford, as he felt it difficult to reconcile it with the general ideas of atomic structure

then favoured by physicists. Indeed, it was not the first, nor has it been the last time, that Rutherford's critical judgment and intuitive power have called forth a revolution in science by inducing him to throw himself with his unique energy into the study of a phenomenon, the importance of which would probably escape other investigators on account of the smallness and apparently spurious character of the effect. This confidence in his judgment, and our admiration for his powerful personality, was the basis of the inspiration felt by all in his laboratory, and made us all try our best to deserve the kind and untiring interest he took in the work of every one. However modest the result might be, an approving word from him was the greatest encouragement for which any of us could wish.

When the War broke out, the little community in his laboratory was dispersed. Having, however, then taken up a lecturing post in Manchester, I had the opportunity in the succeeding years of witnessing the undaunted spirit and never-failing cheerfulness of Rutherford even in the most difficult times. Although the study of the more practical physical problems arising in connexion with the defence of his country took up practically all his time and energy in those years, he could still towards the end of the War find leisure to prepare for, and finally accomplish, perhaps his greatest scientific achievement, the transmutation of an element through the disintegration of the atomic nucleus by impact with α-rays; an achievement which may be said indeed to open up a new epoch in physical and chemical science.

Just at this time Rutherford was, on Thomson's retirement, offered the directorship of the Cavendish Laboratory as his unrivalled successor. I remember on a visit to Manchester during the Armistice hearing Rutherford speak with great pleasure and emotion about the prospect of his going to Cambridge, but expressing at the same time a fear that the many duties connected with this central position in the world of British physics would not leave him those opportunities for scientific research which he had understood so well how to utilise in Manchester. As everybody knows, the sequel has shown that this fear was unfounded. The powers of Rutherford have never manifested themselves more strikingly than in his leadership of the Cavendish Laboratory, the glorious traditions of which he has upheld in every way. Surrounded by a crowd of enthusiastic young men working under his guidance and inspiration, and followed by great expectations of scientists all over the world, he is in the middle of a vigorous campaign to deprive the atoms of their secrets by all the means which stand at the disposal of modern science.

The Cavendish Laboratory: 1876–1900.

By Sir Richard Glazebrook, K.C.B., F.R.S.

IT is fifty years since I first entered the Cavendish Laboratory. Eight years previously physics, in the form of questions on heat, electricity, and magnetism, had become a part of the Mathematical Tripos, and in 1869 a syndicate had reported in favour of founding a special professorship to take charge of these subjects, providing him with a laboratory, a demonstrator, and apparatus to make his teaching practical. Two years later, in 1870, the Duke of Devonshire offered to provide the material part of the scheme "so soon as the University shall have in other respects completed its arrangements for teaching Experimental Physics."

Clerk Maxwell became professor on March 8, 1871, and the laboratory was handed over to the University by its donor, the Chancellor, on June 16, 1874, confident, as he said, "that within its walls researches will be carried out which will advance to no small extent the fame of our ancient University." A prophetic statement; for within those walls have worked Maxwell, Rayleigh, Thomson, and Rutherford, men whose names will ever be landmarks in the history of British physics.

To complete the history of the building, a large ground-floor room for the practical instruction of the M.B. students was added in 1893, while in 1906 Lord Rayleigh offered to devote the greater part of the Nobel prize awarded to him to defraying much of the cost of a new wing; this was completed in 1908. In more recent years the erection of the new engineering laboratory has set free buildings and space adjoining the Cavendish, and a further important extension has been added to the laboratory.

To go back, however, some fifty years. In those days students came slowly. W. M. Hicks was the first. Wm. Garnett had become demonstrator on the opening of the laboratory. J. E. H. Gordon was the first to submit for publication the results of a research conducted at the Cavendish in a paper on the magnetic rotation of water, read before the Royal Society in 1875. When I became a student along with W. N. Shaw and Poynting in 1876, Chrystal and Saunder were at work on the verification of Ohm's law; Schuster joined a few months later. W. D. Niven had an investigation in progress, and soon afterwards Donald MacAlister carried out a modification of the well-known Cavendish experiment on the inverse square law of

electrical attractions. Poynting began his classical research on the determination of the constant of gravitation during this same period.

Experimental physics was a novel subject at Cambridge. In the Mathematical Tripos of 1876, Lord Rayleigh was an examiner and had set a question on the Wheatstone bridge method of measuring resistance, with an easy rider. I had read carefully much of Maxwell's "Treatise on Electricity and Magnetism," published in 1873, but, alas, had paid no attention to Chapter xi. on "The Measurement of Electrical Resistance." My coach had originally marked it with a large O—omit. It is true he had corrected this later, but I had no idea of what was meant by Wheatstone bridge. My annoyance was great when, on my first visit to the Cavendish, Maxwell himself explained it and I realised the simplicity of the question I had passed over.

In those days there were no organised classes in practical physics. Maxwell set us on to gain acquaintance with some instrument, to adjust it to get it to work and make some measurements. Among my earlier experiences I was told off to measure at regular intervals the E.M.F. of a battery of tray Daniell cells used by Chrystal and Saunder. The instrument with which I had to do this was the large pattern of Thomson's absolute attracted disc electrometer, a horrid monster which few students of to-day have ever seen even in a text-book, and fewer still have used.

However, I soon became absorbed in optical work. Stokes, in his well-known report on optical theories, had described a method of determining the form of the wave surface in a crystal and had applied the method to Iceland spar. He encouraged me to attempt the same for a biaxial crystal and lent me the goniometer he had used. Schuster obtained for me from Hilger a suitable piece of aragonite and the work went on for some time in a dark room at the top of the Cavendish; eventuating in a paper published in the *Philosophical Transactions* for 1878, in which it was shown that Fresnel's wave surface was consistent with experimental results to a very high order of accuracy.

During the progress of the work Maxwell was a frequent visitor, interested in its progress, and ready to help. In the case of a serious difficulty he would remark that others had been asking him questions and he must take time. On one occasion I remember him explaining how thick the crust was in which their

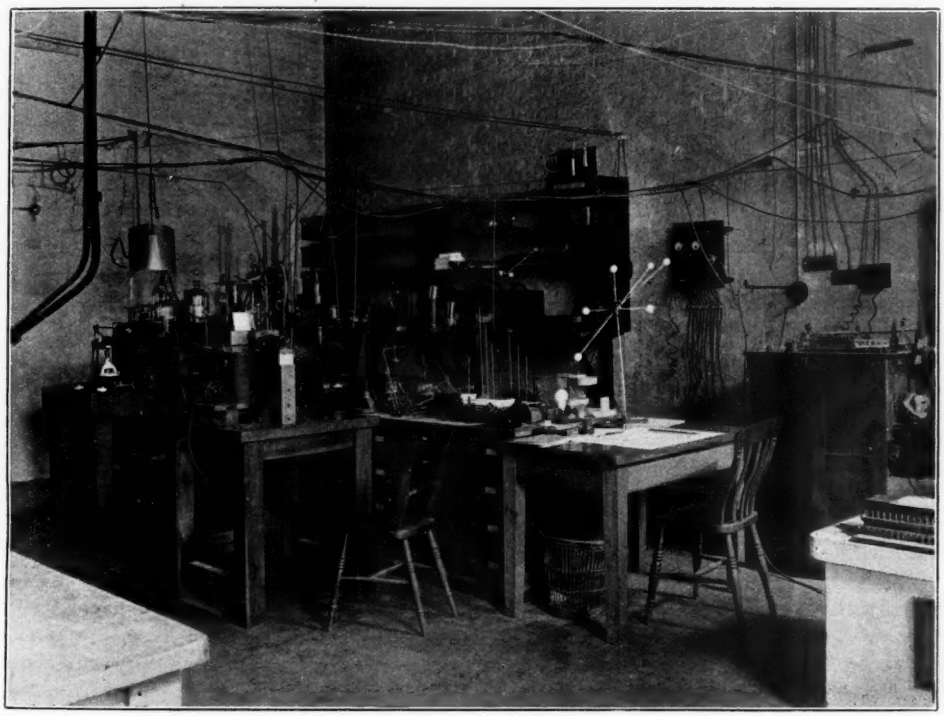


Fig. 7.—General view of Sir Ernest Rutherford's research room. On the extreme left is the electromagnet and microscope used in experiments on the artificial disintegration of some of the elements by bombardment with α-rays. On the extreme right is part of the pillar used by Lord Rayleigh in his determination of the value of the ohm and the ampere. This room was occupied for more than twenty years by Sir J. J. Thomson, for his experimental researches.

questions had encased his brain, but the next day he turned up, accompanied as usual by his dog, with the remark, "If you will alter your arrangements in such and such a way you will find your difficulties disappear," and of course they did.

Those were happy days; there have been many since at the laboratory, but the study of a new subject was full of interest, and the gradual realisation of what experimental investigations with the view of the improvement of material knowledge involved, added much to the pleasure of life and fitted one more and more for the teaching work to come in the future.

In 1879 came a change; Maxwell's health gave way during the Easter term of that year, and after a painful illness he died on November 5. The professorship was continued—it was to terminate originally with the tenure of office of the first professor unless the University determined otherwise—and Lord Rayleigh was appointed on December 12, 1879. He had accepted the post for a limited period—five years—and began work early in 1880. He was anxious from the first to organise regular teaching in practical physics, and appointed Shaw and myself as demonstrators soon after his election. The notice of the work for the Michaelmas term, 1880, after announcing lectures by the professor, by Dr. Schuster on radiation, and by Shaw and myself, continues: "The laboratory will be open daily . . . for those who have had the necessary training. Courses of demonstrations will be given during the Lent term on heat and advanced electricity, and magnetism, and during the Easter term on light, electricity, and sound."

The scheme was something of an innovation in one important detail. Shaw and I were college lecturers, receiving nearly all our remuneration from our colleges; we were among the first to give college lectures in a university building. Hitherto there had been a marked distinction between the lectures of a professor and college teaching; the growth of the natural sciences brought a change. Various colleges still had their own classes and laboratories; at Trinity, Coutts Trotter lectured on physics in college. In physiology and comparative anatomy, however, Foster as Trinity prælector, Frank Balfour and Sedgwick as college lecturers, worked in university buildings. It was clearly out of the question for each college to have its own staff and laboratory in all the new subjects, and so the plan of giving university recognition to college lecturers was gradually evolved, a plan which this year the new statutes have made universal; the formal lectures in all subjects are now given by university officers.

The task we had undertaken was a novel one for us both. Shaw had worked at Freiburg with Warburg and at Berlin under Helmholtz, but at neither place was there organised class teaching in practical physics. My experience had been gained solely as a research student under Maxwell. There were no text-books properly so called. We drew largely on Kohlrausch's "Physical Measurements" and Pickering's "Elements of Physical Manipulation." Pickering at the Massachusetts Institute of Technology had organised a method of instruction which we followed in great measure. There were, it is true, physical laboratories under Wm. Thomson (Lord Kelvin) at Glasgow, Carey Foster at University College, London, Adams at King's College, and Clifton at Oxford. But the time given to practical physics was short, the means of instruction scanty, and there was comparatively little endeavour to teach the principles of physics to a student by means of experiments which he performed himself.

Gradually the system developed; note-books descriptive of each experiment were written, and these in due time became Glazebrook and Shaw's "Practical Physics." The students were taught to describe in their own note-books their work, and enter up their observations; in the advanced class they were expected to read up the theory referred to and work to a high degree of accuracy. The material on which we drew for students was of a very high order. In my class on advanced electricity in 1881, I find the names among others of J. M. Dodds, S. L. Hart, W. B. Allcock, Edward Hopkinson, A. R. Forsyth, R. S. Heath, J. Ryan, and R. Threlfall.

Above all we had the wise counsel and cordial help of the professor: the course was planned and the syllabus arranged after careful counsel taken with him. We could go to him for advice in case of difficulty, otherwise we were free to teach as seemed best and to develop the work in the directions which seemed most desirable. In due time we had efficient helpers, Newall, Wilberforce, Searle, Skinner, Threlfall, Fitzpatrick, Callendar, Whetham, and McConnel, to name only a few, though some of these belong to a later period than that I am now describing.

It was, however, not merely the organisation of teaching which gave interest to the work. The laboratory began to be, to an extent as never before, a centre of new knowledge. A glance at Lord Rayleigh's collected works will show the importance of his output during the five years' tenure of the chair. The experiments of Rowland and Joule had led definitely to the result that the resistance coils constructed by Fleeming Jenkin and Maxwell in 1863 to represent the ohm (10^9 C.G.S. units of resistance) were in error. The Standards Committee of the British Association was reconstituted at Swansea in 1880, and Lord

Rayleigh at once began to take an active part in its work. His first series of experiments with the rotating coil of the original committee, carried out with Schuster, confirmed the fact that there was an error in their determination. Further experiments with a new rotating coil in 1882, and by the method of Lorenz, with the assistance of Mrs. Sidgwick, in 1883, led to definite values from which there has been little variation up to the present. Meanwhile he had arranged that I, as secretary of the B.A. Standards Committee, should undertake to test and issue certificates for resistance coils, and this work went on at the laboratory for the next fifteen years, being ultimately transferred to the National Physical Laboratory.

Throughout this period, and indeed until the end of his life, Sir William Thomson was a frequent visitor to the laboratory, enthusiastic always, keenly interested in the experiments in progress, and full of helpful encouragement for us all.

Work on the electrochemical equivalent of silver, and the E.M.F. of a Clark cell, followed quickly. At a later date Lord Rayleigh took an active part in designing the standard ampere balance for the Board of Trade. He acted as president of the International Electrical Conference held in London in 1908 at which the definitions of the International Ohm, Ampere, and Volt were agreed to by delegates representing Great Britain, certain of her colonies, and twenty-two other countries. The results then reached find their main support in his work[1] during the years 1881–1885.

Some of the apparatus used in these and in the earlier experiments of Maxwell is shown in Fig. 8,

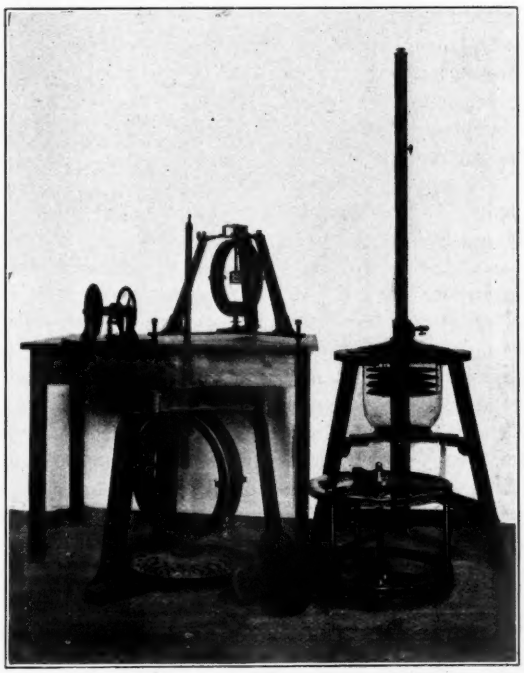


Fig. 8.—Some classical apparatus. The coil on the table is the original spinning coil of Maxwell and Fleeming Jenkin used for the determination of the ohm (1864); it is now on loan at the Science Museum, South Kensington. The coil below is the spinning coil of the late Lord Rayleigh, with stroboscopic attachment, used for the determination of the ohm (1882). The coils on the right were made by Chrystal and used by Glazebrook in his determination of the ohm. On the right is Maxwell's apparatus for measuring the viscosity of air at different pressures. A phonic wheel of Rayleigh's stands to the left on the table.

which is reproduced in "A History of the Cavendish Laboratory, 1871–1910," published by Messrs. Longmans, Green and Co., to which volume I have referred for much in this article. At about the same period the original resistance coils of the B.A. had been brought to the laboratory, and Fleming commenced his long series of electrical researches by a careful intercomparison of these. The Darwins—George and Horace—were at work on the lunar disturbance of gravity. Schuster, in addition to assisting in the electrical experiments, continued his investigations into spectroscopy and radiation. J. J. Thomson's paper—the first dated from the Cavendish Laboratory—"On some Electromagnetic Experiments with open Circuits," appeared in the *Philosophical Magazine* for July 1881. In the previous April his paper on the electric and magnetic effects produced by the motion of electrified bodies had been published; in it he demonstrated that a moving charge will act as though it possessed mass of amount $\frac{2}{3}e^2/a$, where e is the strength of the charge supposed to be treated as a small sphere of radius a. Other papers rapidly followed, while in 1883 a determination of the number of electrostatic units in the electromagnetic unit of electricity was printed in the *Philosophical Transactions of the Royal Society*, and a paper on a theory of the electric discharge in gases in the June number of the *Philosophical Magazine*.

Shortly before his death, Maxwell had called Shaw back from Berlin to undertake work for the Meteorological Council, and thus began, with a paper on the measurement of temperature by water vapour pressure (1883), followed by his report on hygrometric methods (*Phil. Trans.*, 1888), that long series of investigations which has gained him recognition as the leader of modern meteorologists.

My own work on the form of the wave surface of

[1] It may be of interest to compare the values for the ohm (109 C.G.S. units of resistance) expressed as the length of a column of mercury found at the Cavendish Laboratory in 1882 and 1883 with the most recent values.

1882	Rayleigh	106·26 cm.
1882	Glazebrook	106·25
1883	Rayleigh and Sidgwick	106·24
1913	F. E. Smith	$106{\cdot}24_5$
1920	Grüneisen and Giebe	$106{\cdot}24_6$

biaxial and uniaxial crystals was continued in 1886 by McConnel in his paper on quartz. I was diverted from optical work by the duties which came to me on my appointment in 1882 as secretary of the Electrical Standards Committee. Chrystal, when at the laboratory, had constructed a ballistic galvanometer and wound a pair of large coils for a determination of the ohm by a modification of Kirchhoff's method. With the aid of two of the senior students, Dodds and Sargant, these were set up and a series of determinations made, with the result already given, practically identical with that obtained by Lord Rayleigh. These same coils were used by him for his Lorenz experiments, and the close concordance of the results is of importance as an error in the calculated value of the mean radius of the coils would affect them in opposite directions.

At the end of 1884 Lord Rayleigh resigned and J. J. Thomson became professor. The work of teaching continued on much the same lines. The new professor realised no less fully than his predecessor the value of practical physics not only as an engine of research but also as an instrument of education.

Thomson, assisted by Threlfall, at once began that series of experiments on the discharge of electricity through gases which has occupied him until now. Newall returned to Cambridge and shortly afterwards became demonstrator in succession to Shaw, who in 1887 was made tutor at Emmanuel College. About this time Newall, Threlfall, Cassie, Wilberforce, Fitzpatrick, Callendar, and Chree were at work on various problems, while Olearski and Natanson were the first of the long series of research students attracted to Cambridge by the growing fame of the professor.

An addition of some importance was made to the laboratory teaching in 1888, when practical classes for medical students were commenced. For some time previously a viva voce with instruments had been part of the first M.B. examination, and the introduction of more definite teaching suited for the examination became desirable; Fitzpatrick took charge of this. Meanwhile the other classes continued to increase, and as the teaching of practical physics spread to the schools it was necessary to devise fresh experiments of a somewhat more advanced character. Searle became demonstrator in 1891 and threw himself with great zeal into this work.

In 1895 a change of far-reaching importance was made in the University regulations. Up to that date research students from other universities had no recognised position in Cambridge; some few there had been, admitted to the laboratories by the courtesy of individual professors. From this time onwards graduates of other universities, and in some cases students who were not graduates, could enter the University for purposes of research if they satisfied the proper authorities of their competence. They could obtain a degree at the end of two years if they submitted to the Board of Studies a thesis which in the opinion of the Board was "of distinction as a record of original research." In the October term after these regulations were passed, Rutherford, Townsend, and McClelland came as the first students; their number is now legion.

Rutherford's earliest work was on a detector of wireless waves. Hertz's experimental verification in 1887 of Maxwell's theory of electromagnetic waves had been received with great interest when first published. We knew the theory to be true—it was Maxwell's—and to have it verified by direct experiment had been no small satisfaction to Maxwell's pupils. I well remember some time in 1896 being taken by Rutherford into his room to see his magnetic detector at work receiving a message from, I think, the observatory.

Meanwhile Thomson had continued his work on the passage of electricity through gases, and the discovery of the Röntgen rays in 1895 gave him and the band of students he had collected a powerful method of investigating the subject. In 1897 came the discovery of the electron. Perrin in 1895, and Thomson two years later, had proved that the cathode rays of Crookes consisted of a stream of particles each carrying a negative charge. Thomson in 1897 measured the ratio of the mass of each particle to its charge and also the velocity with which the particles travel. The quantity, m/e, was found to be much smaller than would be the case if m represented the mass of the hydrogen atom and e its charge. Moreover, its value was independent of the nature of the gas in the tube, and he concluded that we have in the cathode rays a condition "in which all matter that is matter derived from different sources such as hydrogen, oxygen, etc., is of one and the same kind; this matter being the substance from which all the chemical elements are built up."

In the same year, 1897, Kaufmann and Wiechert determined independently the value of the ratio m/e, arriving at similar results. Schuster in 1884 had suggested that if the cathode rays consisted of charged particles it was possible to determine the ratio e/m by a method practically identical with that employed later by Kaufmann, and in 1890 published results of experiments which led to limits for the value of e/m for the cathode rays.

In the following year Thomson showed that the charge e carried by one of his corpuscles or electrons was the same as that of an atom of hydrogen, and from this it followed that the mass of the electron is about 1/1800 of that of an atom of hydrogen. The

existence of the electron, the fundamental unit of negative electricity, became an accepted fact, and the long series of modern discoveries connected with the names of Thomson, Rutherford, Bohr, and their pupils was the result.

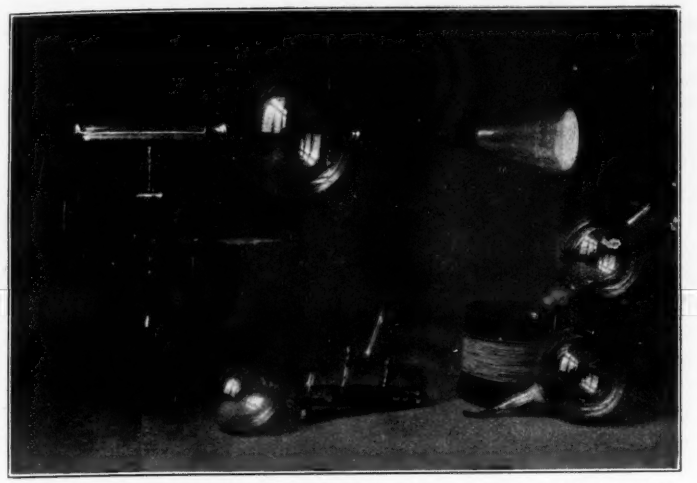


FIG. 9.—Apparatus used by Sir J. J. Thomson in his investigations. The upper tube is an early form of positive ray tube for visual examination of the parabolas. The lower, left hand, tube is one used to show the electrical deflexion of the cathode rays in 1897. By measuring the deflexion of a pencil of rays in an electric as well as in a magnetic field, the velocity and the ratio e/m for the electron were determined. On the right are examples of electrode-less discharge bulbs.

Thomson and his pupils continued this work. McClelland examined the Lenard rays which appear on the opposite side of a thin sheet of metal on which the cathode rays impinge, and showed that they were identical in properties with cathode rays. Stokes had suggested that the X-rays of Röntgen consisted of pulses set up by the sudden stoppage of particles carried in the cathode rays. Thomson investigated the nature of the effects produced by such a sudden stoppage, proving that it would give rise to a thin pulse of very intense electric and magnetic force travelling out from the particle with the velocity of light. He later applied this theory to explain the secondary radiation produced when X-rays fall on matter. Langevin, the first of the foreign students to enter the laboratory under the new regulations, investigated the character of this secondary radiation and published his results in his thesis for the degree of Doctor of Science in Paris.

New knowledge grew rapidly. Rutherford, Townsend, H. A. Wilson, Zeleny, Strutt, and others contributed. C. T. R. Wilson's paper on the condensation nuclei produced in gases by the action of various rays, published in the *Phil. Trans.* for 1899, proved of far-reaching importance. As time went on the influence of the Cavendish researches spread to other universities. Rutherford carried it across the Atlantic to Montreal, Townsend to Oxford, McClelland returned to Dublin, Langevin to Paris, while others came to take their place.

My days at Cambridge closed with my appointment as Principal of University College, Liverpool, toward the end of 1898. Formal connexion with the laboratory had ceased some two years earlier when I had undertaken administrative work for my College. The last experimental investigation I helped to carry out was undertaken along with Lodge, who spent a summer vacation in Cambridge, as an essay to be submitted for a prize offered by Elihu Thomson. The investigation included a determination of v by means of the measurement of the frequency of the oscillations in a circuit of known capacity and inductance, and was published in the Stokes Memorial volume

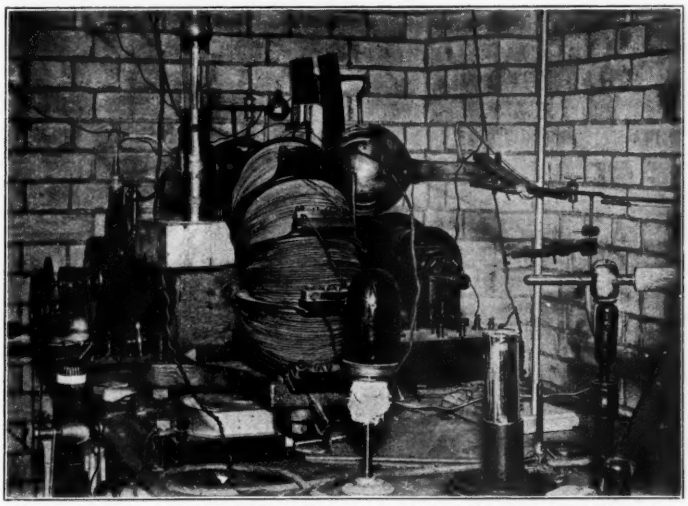


FIG. 10.—Apparatus for the analysis of positive rays by Sir J. J. Thomson's 'parabola' method. This was the first made in which the photographic plate could be lowered into position by a vacuum-tight winch. It was in this room, with a slightly modified apparatus, that the duplicate parabolas of neon were first photographed, in 1912.

issued in 1899. Up to that time I had been closely connected with the Cavendish for nearly twenty-five years and have written of things I knew. I must conclude now and leave to others more intimately acquainted than myself with the progress of the last quarter of a century to tell in detail what that progress has been. Thomson continued his work until 1918, through the troubled period of the War, when the researches of the Cavendish assisted to the full in the solution of many problems. In that year Dr. Butler, the revered Master of Trinity, died, and Thomson was chosen by the Crown as his successor. He resigned the Cavendish professorship with its charge of the laboratory, and the University created for him a professorship of physics which will cease with his tenure of the post. Rutherford followed as professor, and under his genial direction he and his 'boys' from day to day increase the sum of natural knowledge by their amazing results and add in no small degree to the fame and influence of the Cavendish Laboratory, thus verifying the forecast of its founder more than fifty years ago.

Research Work in the Cavendish Laboratory in 1900–1918.

By Prof. J. A. Crowther.

THE progress of science and the life of a laboratory have little connexion with the artificialities of chronology, and the beginning of the century does not coincide with any natural division in the work of the Cavendish Laboratory. If, for the sake of clearness, we must make a division in what was fundamentally a natural and continuous growth, our most convenient date will be the year 1903, and our obvious landmark the publication of the first edition of Thomson's monumental book, "Conduction of Electricity through Gases." Here we find for the first time a complete and logical account of the solid achievements of the previous eight or ten years of remarkable activity. Here also we find the foreshadowings of the ideas which were to direct and regulate a large part of the work of the succeeding years.

In the main the problems attacked in the years before 1903 were those of the mechanism of the conduction of electricity through gases. Some of these, it is true, were carried over into the succeeding period. Some, perhaps, have not yet been fully solved, but by 1903 the foundations of the subject had been well and truly laid. It had been proved, beyond doubt, that the conductivity imparted to gases by various agents, such as X-rays, the rays of radium, and ultra-violet light was due to the formation of charged particles or gaseous ions in the gas, and many of the properties of these ions had been determined. Rutherford had determined their mobility; Townsend had determined their rate of diffusion through various gases; C. T. R. Wilson had shown that they could serve as nuclei for the condensation of water in a supersaturated space. Thomson had discussed their motion, and shown that in all probability they consisted of single molecules of the gas in which they were formed.

In a closely allied subject Thomson had demonstrated experimentally the existence of the electron, or negative corpuscle as we then called it, and had determined its few simple properties. The measurement of the electronic charge actually falls within the present century, though preliminary experiments had been made somewhat earlier. Determinations were made independently by Thomson and by H. A. Wilson, by somewhat different methods, and were published in 1903. During the same period Zeleny made new measurements of ionic mobilities, which when combined with further measurements on the diffusion of ions by Townsend established conclusively the identity of the charge on a gaseous ion with that on a monovalent ion in solution. These experiments mark the culmination of the first epoch.

When I commenced research work in 1905 the second epoch of activity was already well under way. With the exception of C. T. R. Wilson, who in the advanced laboratory was engaged in imparting to a new generation of students those experimental arts of which he is a master, the first group of workers had all dispersed. Their work, as collected in the pages of "Conduction through Gases," formed our text-book, and was the foundation on which we hoped to build.

It was a very happy, though crowded, community to which I had the honour of being admitted. The space available for research had not been extended since the laboratory was built, and what had been adequate for ten or twelve, the average number of research workers before 1903, was becoming a tight fit now that the numbers had risen to an average of twenty-five. A competent observer records that there was more physics to the square centimetre in the Cavendish than anywhere else in the world, and the resources of the laboratory in the way of apparatus were sometimes severely tested, though I cannot recall that they ever broke down. The oft-repeated legend that we worked entirely with sealing wax and string, though flattering to our technique, has little foundation in fact. The apparatus for the new experiments which we wished to make did not exist. It had to be invented and made on the spot. An adequate workshop, staffed with extraordinarily competent and helpful mechanics, with F. Lincoln as chief, carried us through the worst of our difficulties; while Everett, the professor's private

assistant for so many years, was always willing to lend a hand in the worst intricacies of glass-blowing. For the rest there was the spirit of cheery fellowship and good comradeship which extended from the professor

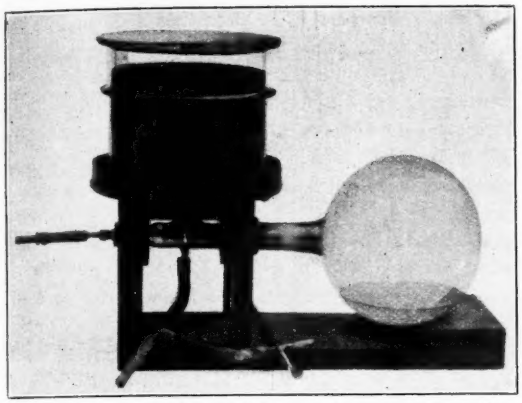


FIG. 11.—Prof. C. T. R. Wilson's original expansion apparatus for making visible the tracks of ionising particles. It was made in the Cavendish Laboratory in 1911 and is the apparatus with which his photographs of the tracks have been obtained.

to the rawest recruit, and bound us all into a genuine community.

It is clearly impossible to attempt to give any adequate account of the activities of so many workers, extending for so many years, within the bounds of a single article. A mere list of publications would fill more space than I have at my disposal. I can only outline the general trend of discovery in the laboratory as I saw it, and many excellent pieces of research such as those of Burton on colloids, of Wellisch on ionic mobilities, of Vegard on osmosis, and of C. T. R. Wilson on atmospheric electricity, must pass unnoticed. This is the more unfortunate in that it may tend to accentuate the erroneous impression that the research work of the Cavendish has been entirely specialised and one-sided. This has never been true. Workers with ideas in any branch of physics have always received a warm welcome, and the Cavendish has rarely been without them. The majority of the research workers, however, came to the laboratory as young students, to learn their job, and it was natural and inevitable that their work should be inspired and directed by the genius of Thomson, and should be correlated to the subjects which he had most in mind.

The dominant problem from 1903 onwards was that of the structure of matter, to which the way had been opened by the discovery of the universality of the electron. Thomson's main theoretical contributions to the subject appeared in 1903, 1904, and 1906, and dealt with possible arrangements of electrons to form atomic structures, and the properties—electrical, magnetic, and optical—which might be expected to result from these arrangements. The substance of these papers reappeared, in a somewhat more popular form, in "Electricity and Matter" and the "Corpuscular Theory of Matter." To bring these ideas to the touchstone of experiment was, perhaps, the main preoccupation of the laboratory until the year 1912.

A study of electrical conductors seemed to offer a hopeful line of approach. If the current through a metallic conductor is carried by electrons, which share in the thermal agitation of the molecules, some of them should escape from the surface if the temperature is sufficiently raised. Preliminary experiments on the subject were made by O. W. Richardson so early as 1901, and some of the theoretical consequences were discussed by Thomson in the following year. The subject was hotly pursued by O. W. Richardson in the Cavendish until 1906, and afterwards at Princeton, and has developed into the important branch of physics known as thermionics. Thomson, Horton,

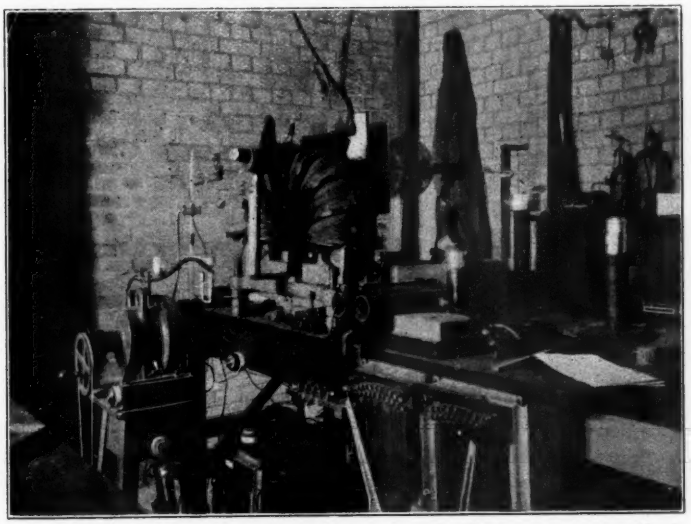


FIG. 12.—The first mass-spectrograph set up by Aston in 1919. This instrument was capable of comparing the masses of atoms and molecules with an accuracy of 1 in 1000. By its means, in five years, more than fifty elements were resolved into isotopes or shown to be simple.

and others also made contributions to the subject. The preliminary work necessary to disentangle the genuine effect from the many spurious phenomena which accompany it was tedious and harassing, but important and valuable results were eventually obtained.

The question of the radioactivity of ordinary materials, which was being investigated in the laboratory about this time by N. R. Campbell and A. Wood, also had its bearings on the main problem. Difficulties were met with in eliminating the effects of penetrating radiations from outside sources and the possibility of small traces of radium in the materials employed, and the results were indeterminate, except in the case of some of the alkali metals, which were found to emit appreciable quantities of β-radiation. Experiments made by Kleeman on the relative ionisation produced by different ionising radiations in gases, by Laby on the total number of ions produced by the complete absorption of the α-rays from uranium in different gases, and by Kaye on the output of X-radiation from targets of various metals, while important in themselves, also had a more or less direct bearing on the main problem of the atom.

In 1906 Thomson returned to the attack and gave the theory of two important methods of estimating the number of electrons in the atom of a given element. One method suggested was to measure the diffusion of a parallel beam of β-particles in their passage through thin sheets of matter. Assuming that the deflexions are due to collisions with the electrons in the atoms through which they pass, a determination of the scattering of the particles should provide an accurate estimate of the number of scattering centres. A second method consisted in measuring the intensity of the scattered X-radiation from a given radiator when subjected to a beam of primary X-radiation. Here again, assuming the correctness of the classical theory of the phenomenon which had already been given by Thomson, it should be possible to deduce the number of scattering centres from the intensity of the radiation scattered. The experimental working out of both these methods fell to my lot, and kept me occupied from 1906 to 1911. The results showed that the number of electrons per atom was quite small, approximately of the order of the atomic weight, and that the main mass of the atom must be associated, not with its electrons, but with its positive charge.

In the meantime Thomson had himself been investigating the positive charge on the atom by various modifications of the method which had proved so successful with the electron. The results of these experiments on positive rays were published in various papers from 1908 to 1914. The importance of the new method lies in the fact that for the first time atoms could be dealt with as individuals, and not merely in aggregations of many millions. The results of these experiments, together with those of Aston (1919–1925) which led to the discovery of the existence of isotopes and the whole number law for the masses of the atoms, are too fresh in the memory to need further description.

In the meantime new ideas were afloat. The phenomena of secondary corpuscular radiation excited by X-rays, as studied by Innes, and of photo-emission (Hughes and others) were becoming more and more impossible to reconcile with the classical wave theory of light. The theory that the wave front was discontinuous was put forward by Thomson so early as 1907, and experiments to test the matter were made by N. R. Campbell, and by G. I. Taylor in 1909. Laue's discovery in 1912 of the diffraction of X-rays by crystals placed a new weapon in the hands of physicists, and by 1913 W. L. Bragg, working in the Cavendish, had already employed it to measure the distance apart of the atoms in a rock-salt crystal, and to deduce an absolute value for the wave-lengths of various X-radiations. A little earlier (1911–1912), C. T. R. Wilson, by an ingenious modification of his early experiments on the deposition of water vapour on charged ions, had evolved a method of making visible the actual tracks of ionising particles in gases, a method the potency of which has not yet been fully exploited, although it is now in use in many laboratories in all parts of the world. Whiddington, by his experiments on the excitation of characteristic X-radiation by cathode rays (1912), was already approaching the quantum theory of X-ray phenomena. At the commencement of 1914 the stage seemed fully set for a new outburst of activity which should rival even the early days of the laboratory under Thomson.

Alas, however, the world stage was set for quite a different drama, and its research workers found problems for solution far different from those which they had been expecting to attempt. When the laboratory began to fill up again in 1919, it was to learn that its professor had found the double duty of directing at one and the same time a great laboratory and a great college too much for one man's strength and time, and the Cavendish professorship was resigned. This is not the end of the story. Sir J. J. Thomson still continues his theoretical and experimental researches with unabated vigour and success, and under its new chief, Sir Ernest Rutherford, the Cavendish goes on to new problems and new triumphs. But these changes may conveniently mark the end of the present article, which has already exceeded its allotted span, without doing anything like justice to the many old companions and friends by whose side I have worked in the Cavendish laboratory, and whose faces look down upon me from the framed groups upon my study walls. Having worked in the Cavendish they will understand the impossibility of the task I have undertaken, and being Cavendish men they will forgive.

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